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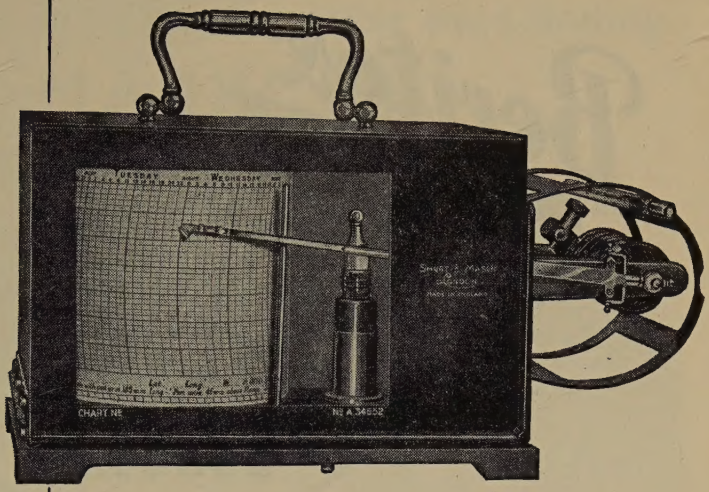

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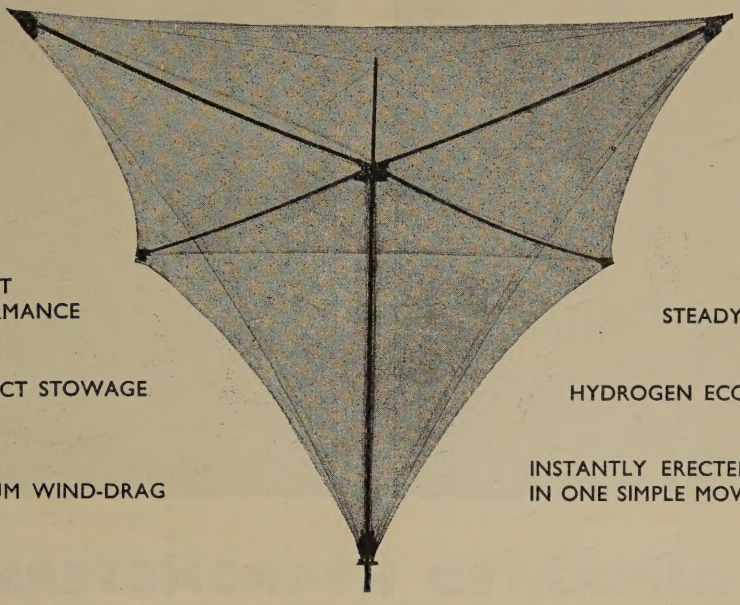

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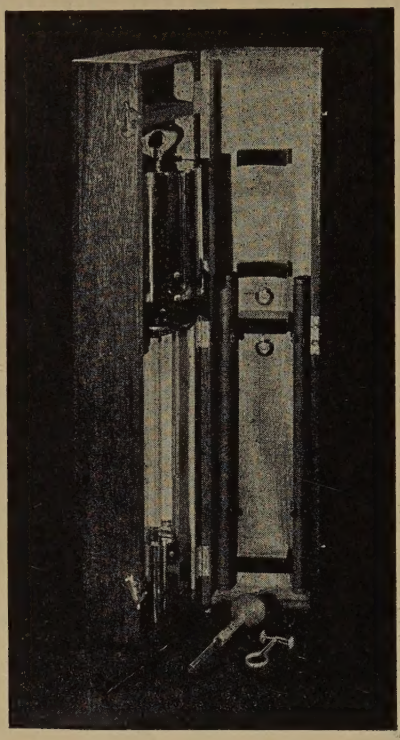

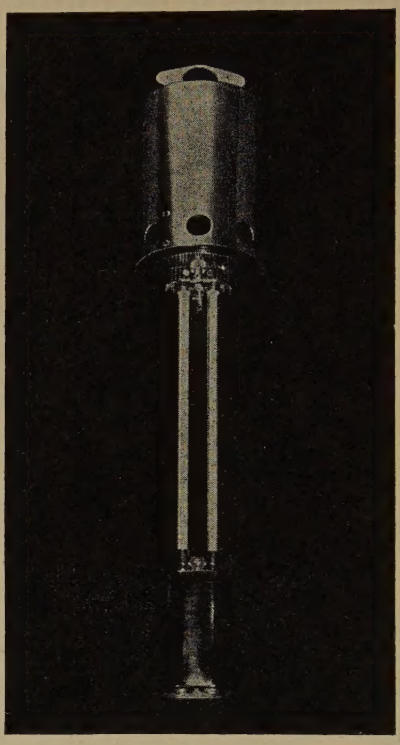

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REVIEWS OF MODERN METEOROLOGY—12

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The problem of tropical hurricanes

By T. BERGERON
University of Uppsala

(Based on a lecture given to the Royal Meteorological Society on 28 June 1950; there is no reference to literature after 1951)

1. Introduction

Tropical hurricanes are much more rare than extra-tropical cyclones, but they are also by far the most powerful wind systems of our globe, at low levels, judged by the product of specific kinetic energy and area, and the total damage caused by such storms. A daily Weather Service based, among other things, on a solution of the cyclone problem is, therefore, even more necessary and useful in the areas frequented by tropical hurricanes than in our latitudes.

By about 1928 the methods and main results of the Bergen School were becoming consolidated as regards their application in temperate and higher latitudes, and especially in Europe. It was then apparent that, if their basic philosophy were sound, these methods should also be applicable to a certain degree to the atmosphere of low latitudes. Their utilization within tropical regions might prove fruitful, leading to a better understanding of many weather phenomena both within and outside this region. Accordingly, in a lecture at the University of Oslo in 1928, I treated the application of front and air-mass methods to the weather of low latitudes, postulating that tropical hurricanes* might develop in connection with a disturbance of the Tropical front,† and that this development would then be analogous to a certain kind of extra-tropical cyclogenesis.

I was, of course, quite aware of the great difficulties in treating the above problem along such lines, since real hurricanes form only over tropical seas where very few, if any, reliable meteorological stations are available, and since the general conditions within these areas differ so much from those over the continents of the temperate zones. However, the existence of at least one more necessary condition for the formation of a hurricane, apart from those already known, the *kinetic*, or mechanical, condition (Dove 1828) and the *convective* one (Ferrel 1856), then seemed, and still seems, to be indicated by the following two facts :

* The term tropical hurricane, or simply hurricane, will here be reserved for a (cyclonically) revolving storm having a diameter of the order of magnitude 10^3 km, being generated between 20°N and 20°S, and at least in some parts attaining wind force 12 (Beaufort scale).

† The existence of this front was demonstrated on the same occasion. There is still no reason for discarding the old term Tropical front (T.F.) and for calling this mechanism the Intertropical convergence zone (I.T.C.Z.). The T.F. has, in fact, all the properties of a *front*: it is a narrow zone of transition of dynamic importance between two high-reaching air masses of vast extent and essentially different origin, involving an appreciable cyclonic shear of motion and some difference of air-mass properties, and being (thanks to the shear and the convergence) a potential zone of origin of cyclonic disturbances. The *amount* of the temperature-difference at a front is not an essential part of the definition; this difference will generally vanish with certain fronts (cf. Raethjen 1938) and near the tropopause (cf. Berggren 1952). On the other hand the two-dimensional convergence is common to all fronts, and is not a special feature of the T.F. (or I.T.C.Z.). The idea of a convergence is a much more general one than that of a front. Viewed three-dimensionally, all typical fronts are kinematically formed, or at least sharpened, and maintained by fields of deformation.

I. The condition for the generation of hurricanes according to the convective theory – a great store of ' lability energy ' (' Labilitätsenergie ' in the sense of Refsdal 1930, pp. 8-9 and Fig. 3, hereafter called ' lability energy ') – is often fulfilled over vast areas of the tropical seas; and the general condition of the kinetic theory, considerable cyclonic vorticity, prevails along the typical frontal zones and within all Lows of sufficient depth on the globe. Yet the generation of hurricanes is very rare even within the areas and seasons defined by the above conditions.

II. On special occasions the regeneration of an old, occluded and filling *extra*-tropical Low leads to the formation, at the centre of the parent cyclone, of a new one which is much smaller and more intense than its parent. Several remarkable cases of this kind had been observed to occur over north-western Europe (cf. below § 10).

Thus, my theory* (cf. Bergeron 1928, pp. 93-94) for the generation of a hurricane postulated one of the two following rare coincidences :

(1) either between the kinetic condition and a rarely-occurring factor which might be a special mechanism capable of transferring and concentrating pre-existing cyclonic vorticity into the area in question,

(2) or between the convective conditions and a rarely-occurring factor which in this case might be a special thermodynamic mechanism, capable of transforming the lability energy into the kinetic energy of an intense cyclonic vortex.

Some kind of combination of the two cases seemed plausible. A wave of the Tropical front would cause a moderately intense but sufficiently deep tropical Low which, (a) would gradually concentrate the cyclonic vorticity (the kinetic condition) and which, (b) together with some front, might release and control an orderly overturning within an air mass containing a sufficiently great amount of lability energy (the convective condition).

This theory also forms the substance of Section 185, on tropical hurricanes, in *Physikalische Hydrodynamik*, 1933 (V. and J. Bjerknes, Solberg and Bergeron). It seems generally to have escaped notice by later writers on the hurricane problem, except by Ch. Deppermann, who visited Bergen and Oslo in 1932 and emphasizes the role of the T.F. in tropical cyclogeneses.

Until recently, research on tropical hurricanes has been concentrated chiefly on fundamental questions other than those considered above. For the period up to 1920 the reader is referred to the very exhaustive, but mainly descriptive, treatise on tropical hurricanes by Mrs. E. V. Newnham (1922), with an introduction on the birth and death of cyclones by Sir Napier Shaw. Even after this time the relevant papers have mostly dealt with mapping the tracks of hurricanes all over the tropical regions and with investigations of the *general conditions* under which hurricanes form and move: factors favouring or suppressing their development, influencing their tracks, etc.

The *structure* of tropical hurricanes has also, to some extent, been studied, with results that have led to a modification of the current theories. With a few exceptions (cf. below, p. 154), the *process of formation* has hardly been treated satisfactorily.

As mentioned above, the structure of hurricanes, and still more the process of their generation, is as yet practically inaccessible to a direct scientific study, because these storms only form and exist in a typical shape over tropical seas, and since they are so small compared to the meshes of the network of observations.

This review cannot, of course, pretend to render a complete or quantitative treatment of the above subject. It will give mainly a series of maps and schematic cross-sections, intended to throw light on some plausible necessary conditions for the formation of tropical hurricanes, and to show a possible process leading, or contributing, to this

* Expounded more fully at the Scandinavian Scientific Congress at Copenhagen in Aug.-Sept. 1929.

formation. Furthermore, some still unsolved problems will be pointed out, and the kind of observations needed for progress in these fields will be indicated.

Contemporary meteorology has in many fields left the *qualitative* stage and tries to treat the problems *quantitatively* by a more or less exact mathematical analysis or by experiments. Regarding the tropical hurricanes, however, the time is probably not yet ripe for this new stage : we must first determine in a satisfactory way what *kind* of atmospheric machinery we are dealing with and its general manner of functioning. This can only be done by a careful analysis of climatic maps (general conditions), of daily weather maps for different levels, and of vertical cross-sections for special cases as well. In carrying out such studies one should consider processes of all orders of magnitude, from those underlying the physics of clouds, to those governing the general circulation.

2. General conditions for the formation of tropical hurricanes

Palmén, in 1948, made a fundamental contribution to our understanding of the tropical hurricanes by a study of the thermodynamic conditions and functioning of the North Atlantic hurricanes. Rather conclusive proofs were attained as to the necessity of the convective condition for this kind of hurricane. (Figs. 2 and 9 in this article are in part borrowed from Palmén's paper, and Fig. 1 is inspired by it).

The global map, Fig. 1, shows (1) that within all those oceans where the conditions are well known, i.e. the Atlantic, the Indian Ocean and the western Pacific, no hurricane-track originates in a region where the mean air temperature is appreciably lower than 27°C, (2) the major part of all oceanic regions with an air temperature above 27·5°C lies within the regions of hurricane origin. Thus, it seems that an air temperature at sea-level of at least 27° is generally a necessary condition for the formation of tropical hurricanes, a circumstance strongly supporting the convective theory. From the regions which are defined by the above conditions, however, we must everywhere exclude, according to the map, (3) the zone between 5°N and 5°S and (4) a coastal zone of about 300 to 500 km width, presumably corresponding to the diameter of a precursory stage of the hurricane. In Fig. 1, therefore, all oceanic regions other than (3) or (4) with an air temperature higher than 27°C or 27·5°C have been shaded.

The coincidence between the different shaded regions and the areas of origin of hurricanes is striking, except in the South Atlantic where no hurricanes occur. This ocean is, however, the only one where the Tropical front (the doldrums) nowhere reaches south of 5°S at any time of the year (cf. the letter M. in Fig. 1 within the Bay of Guinea). Over the North Atlantic, the similarity will be noted between the northern limit of the hurricane source region, indicated by a thick dashed line, and the isotherm of 27·5°C. The map also shows that the formation of hurricanes is greatly affected even by very small continental regions. The western border of the North Atlantic hurricane source region even seems to avoid the Lower Antilles.

The Tropical front of the North Indian Ocean is not shown in Fig. 1 because this front is rather diffuse or uncertain in the average picture of the wind and pressure distribution during the hurricane season. Apart from this case Fig. 1 points, (5), to a certain connection between the Tropical front and the formation of hurricanes. Thus, in the eastern part of the Pacific, the Tropical front never reaches south of 5°N (see M. to the south of Hawaii in Fig. 1), and, as one would expect, no hurricanes are found to the south of the equator. Within the latter maritime region the air temperature, it is true, lies below the critical value of 27°C. But the same is valid also of the hurricane source region in the eastern Pacific to the north of the equator.

The above circumstance, and other deviations from factors (1) to (5) that generally seem to determine the position and extent of the hurricane sources, can be explained in

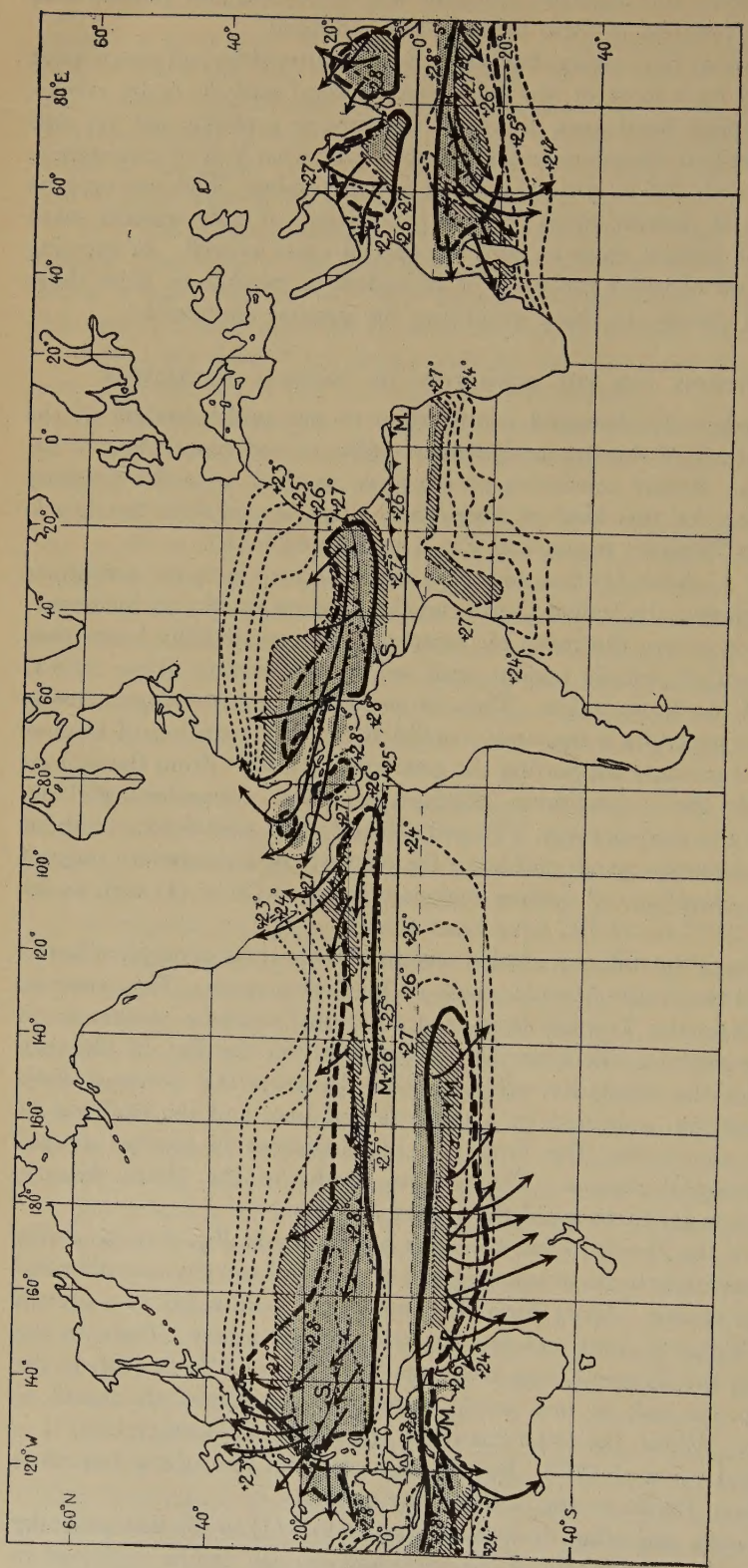

Mean air isotherms at s -1 { September N. Hemisphere / March S. "
Mean position of Tropical front in September.
" " " " " March.
Hurricane track.
Approximate poleward limit of region of hurricane origin.
" equatorward " " " " "
shading indicates mean air temperature + 27.0 to + 27.5°C
shading " " " " > + 27.5°C in September and March respectively, outside the zone 5°N. - 5°S. lat. and > 250 miles from land.

Figure 1. Areas of hurricane formation and tracks of hurricanes during the whole year, the isotherms of Sept. and Mar. for the air at sea-level, and the Tropical fronts in their extreme positions over the oceans. Hurricane areas and tracks from *Lärobok i navigation* (Manual on Navigation), Fig. 314, Stockholm 1945. Isotherms mainly from the ocean maps published by Kon. Ned. Met. Inst., De Bilt 1912 and 1914, and by U.S. Weather Bureau, Washington D.C.

different ways. First, neither the hurricane source regions nor the average air temperatures are sufficiently well known. Secondly, the temperature distribution, even over tropical seas, varies in time and space; it may sometimes, locally, deviate considerably from the average conditions of the month. Thirdly, the lability energy, which is necessary for the formation of a hurricane is not only determined, we know, by the temperature and humidity conditions at the sea surface, but also by their distribution in the upper air, at any rate up to the 300-mb surface, as discussed by Palmén (1948, Figs. 1-3). Finally, we must bear in mind that any occurrence of a hurricane may have been favoured by an abnormally high value of the sea and air temperature, or by an abnormally low value of the upper-air temperature.

Fig. 2 shows, according to Palmén, the mean vertical temperature distribution of the atmosphere over the Caribbean Sea up to the 22 km level, for September and February, and the temperature curves of adiabatic lifting for air starting with the mean sea-level temperature of the month, i.e., 28°C and 25°C, and an assumed mean relative humidity at sea level of 85 per cent. As pointed out by Palmén, this diagram proves that, during September, the convective condition for the formation of hurricanes is abundantly fulfilled – provided that we may neglect entrainment and the warming of the ambient air by the hurricane process. On the other hand, the figure shows that during February no convective formation of hurricanes is possible. These conclusions coincide well with observation since the hurricane season over the Caribbean and the adjacent part of the Atlantic is most decidedly August till October, whereas no tropical hurricanes ever form there during February.

In this connection it is interesting to note that the two main regions where Atlantic hurricanes form in August and September, found by Mitchell (1924) – just off the west coast of Africa and at 60°W – are also indicated on Palmén's map (1948, Fig. 3), which illustrates the average lability energy over the North Atlantic in September.

3. Generalities on the structure of tropical hurricanes

As mentioned above, one encounters enormous technical and other difficulties when trying to study the detailed structure of tropical hurricanes. Both in the stage of formation and when fully developed, the hurricanes exist in their typical form only over the oceans

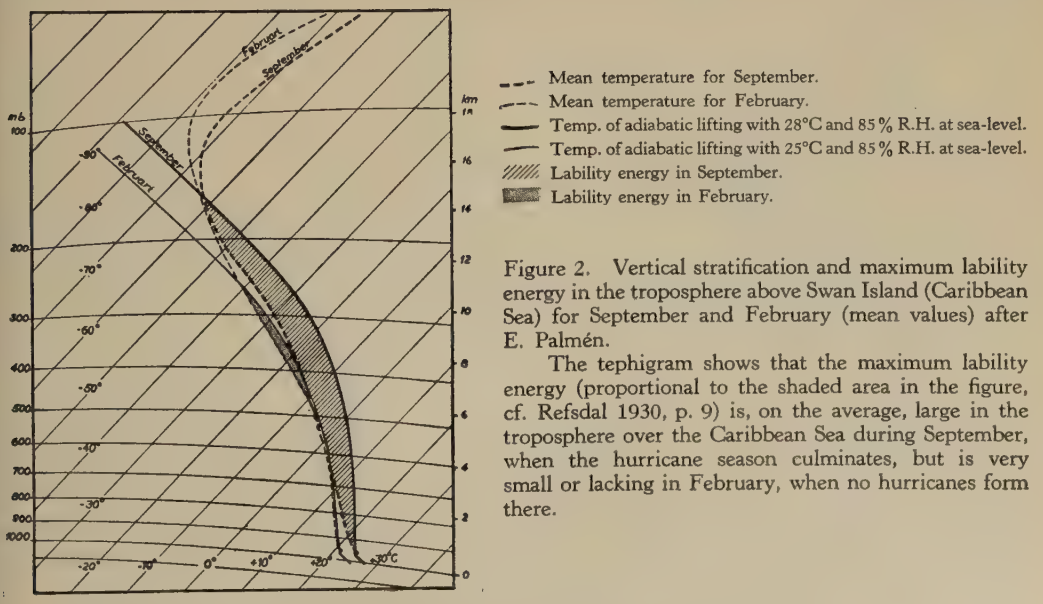


Figure 2. Vertical stratification and maximum lability energy in the troposphere above Swan Island (Caribbean Sea) for September and February (mean values) after E. Palmén.

The tephigram shows that the maximum lability energy (proportional to the shaded area in the figure, cf. Refsdal 1930, p. 9) is, on the average, large in the troposphere over the Caribbean Sea during September, when the hurricane season culminates, but is very small or lacking in February, when no hurricanes form there.

of low latitudes (20° N to 20° S), where good observations from a sufficiently dense network of ships are missing, and where the number of observing island stations and of soundings is extremely small.

Up till now, an *undisturbed* tropical hurricane, in its initial or in its mature stage, has never, to my knowledge, been studied with the aid of synoptic maps analogous to the numerous and detailed maps through which the fronts and the life cycle of cyclones of the Bergen school were discovered. Instead, one has had to be content with using certain tricks in order to replace the missing dense network of observations :

(1) If the structure of the hurricane could be regarded as unchanged for some time, the time-section given by registering instruments could be interpreted as a section in space.

(2) In the absence of continuous records, the observations made at different hours may be plotted on one and the same map in their correct position relative to the centre of the disturbance. This gives, in a way, the best supplement to the very scanty network of observations. During the stage of formation, however, both the above methods are inappropriate since the storm then changes its structure too rapidly.

(3) Of late, reconnaissance flights have also come into use for such purposes, but even this method has its shortcomings. In fact, it is by now obvious that such flights cannot replace a really dense network of complete and absolutely simultaneous observations from fixed points at different levels.

A hurricane often appears as an extraordinarily violent but small vortex – diameter at first even < 100 km – at the centre of an ordinary Low; within the latter, wind forces will generally not exceed force 7. Thus, one may infer that, in the majority of cases, a tropical disturbance is not recognized as a hurricane until some time after it has reached the mature stage. Occasionally this development, or at any rate the real centre, may entirely escape notice, especially when no reconnaissance flights are used. This may explain why even recent investigations of hurricane source regions and tracks over little-frequented oceans sometimes are so contradictory and give so little definite information on the conditions of the formation and motion of these disturbances.

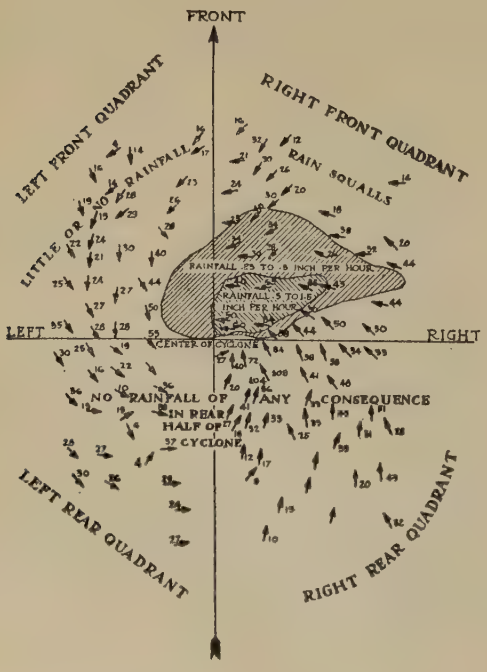


Figure 3. Ground plan of tropical hurricane, derived from four storms having a diameter > 450 mi, after I. M. Cline (1935). The figures at the wind arrows are average velocities in mi/hr. The shaded areas show rainfall intensity.

The famous Far East hurricane of 21 September 1934, the Muroto typhoon, gives an example of the phenomenon mentioned. As it passed over Japan one could clearly distinguish the real typhoon, with extremely close isobars, at the centre of a pre-existing low-pressure region of moderate intensity (cf. Fujiwhara 1939). B. W. Thomson (1951, p. 278) also states that the real typhoon vortex is often surprisingly small.

The Bengal hurricanes, studied by Ramanathan (1935) and others, some of the Philippine hurricanes treated by Depperman, and many of the hurricanes reaching the south-eastern U.S.A., studied by Cline, are trivial in so far as they seem to present a modification of the frontal cyclone model of our latitudes in lower latitudes where humidity is greater and the Coriolis force is smaller. Thus, Fig. 3, from Cline (1935), represents average conditions within four cyclones of more than 450 mi width ($d > 720$ km), and this big diameter indicates in itself that those disturbances no longer were true tropical hurricanes.

The tropical hurricanes reaching Japan, the typhoons, have of course been much studied by Japanese meteorologists (Arakawa, Fujiwhara, Horiguti, etc.), and their devastating effect may be just as great as that of a typical tropical hurricane. The Japanese synoptic typhoon maps are less convincing, however, with respect to our problem, among other reasons because of the relatively high latitudes at which these storms reach Japan (30°-35° N) and the inevitable disturbing effect from the adjacent Polar front. Further, all tropical hurricanes reaching Japan are already fully developed, or even beginning to degenerate, and they are deformed by the mountain ranges of Japan. (Since this article was written H. Arakawa and T. Fujita have published interesting communications on typhoons; some of their results will be treated in a future paper).

4. Large-scale synoptic-aerological studies of hurricanes

The investigations, led by Riehl and others, of tropical hurricanes at Porto Rico and at Guam respectively during the second World War are in some respects specially interesting. For the first time in the history of meteorology there was now a network of temperature and wind measurements in the upper air, available within two main regions of hurricane formation, and the upper-wind observations were comparatively numerous. Everything is relative, however, and owing to the small diameter of a tropical hurricane and its comparatively rare occurrence, even this network was too scanty to be able to reveal the hurricane structure in detail or the process of formation unambiguously.

The synoptic part of these important investigations was mainly based on streamline maps for different levels throughout the troposphere, and they have given valuable insight into the connection between the formation of tropical hurricanes and their tracks on the one hand, and the shape of the upper currents and the distribution of tropical and polar air at different levels on the other. According to Riehl (1948) some of these hurricanes form near the northern limit of the Equatorial westerlies, i.e., at the Tropical front. He also confirms my old theory that a pre-existing moderate cyclonic vortex is a necessary condition, acting as an ordering mechanism for the overturning within an unstable air mass with the great lability energy assumed by the convective theory. Like Palmén and myself, Riehl concludes that the θ_w value of the indraught air in the bottom layer, together with the value of $\partial T/\partial z$, decides whether a tropical hurricane may form and be maintained or not.

Riehl claims, however, that typical hurricanes may also originate within a deep homogeneous easterly current of tropical air, i.e., without any connection with a front. The initial Low then forms within the homogeneous easterly field of motion because of its inherent dynamic instability. Therefore, he maintains that a satisfactory hurricane theory, i.e., a theory which is able to explain all kinds of hurricanes (and especially the violent and symmetric ones), must be able to manage without any front as a necessary

pre-requisite*. Thomson (1951) has found, however, that more than 50 per cent of the major tropical hurricanes in the waters around the Philippines form within 100 km of the Tropical front and Depperman (1936, p. 17) had never experienced any typhoon formed outside the T.F.

5. Attempt at a quantitative hurricane model

The thorough and extensive statistical and synoptic studies of true tropical disturbances in the Far East, carried out in the period 1930-1939 by Depperman at Manila, may already be termed classical. Three different air masses dominate in turn the region investigated, i.e., the Philippines and the waters around them : the easterly trade-wind, the relatively dry and chilly north monsoon from Asia in autumn and winter, and the south-west monsoon, which may occur at all seasons but winter. In the autumn, hurricanes or typhoons often seem to form in the zone of transition between these three main air-currents, the warm and humid south-west monsoon being supposed to supply the greater part of the necessary water vapour for the hurricane mechanism.

None of the weather maps constructed by Depperman, however, is able to show a hurricane at the stage of formation, and the maps are not even convincing regarding the structure of the fully developed hurricanes. Although the Philippines permit the existence of a rather well developed network of stations, distributed over an ocean area, the archipelago itself with its mountains, like Japan, evidently exerts too much of a disturbing effect on the delicate hurricane mechanism (cf. Depperman 1946). In fact, the hurricanes either form farther east, over the Pacific Ocean, or in the China Sea (cf. Thomson 1951).

Depperman stated that the typhoons always form at or near the Tropical front, but that their interior does not contain any fronts in the fully-developed stage. He then threw doubt upon the existence of a downward motion and a warming of the central air column of a generating typhoon. This would imply a reduction of the importance of the hurricane eye, which according to both classical and modern views holds a position in the hurricane that in a double sense is a central one. Depperman even seemed to regard the famous hurricane of 19 to 22 October 1882 over Manila as untypical with regard to the ' eye.' In my opinion, the disturbance just mentioned, which according to the meteorograms consisted of an extremely intense and symmetric vortex around a central region with warm and relatively dry air, must be regarded as in every respect a typical hurricane. In any case, such hurricanes are generally the most violent and long-lived ones. This is, in fact, the type of hurricane that differs most from the ordinary Lows and extra-tropical cyclones, and which cannot be fully described by any structure model known in our latitudes.

According to works by H. Wexler, Depperman, Riehl, Sawyer and others, the structure of a mature hurricane as seen in a vertical cross-section is nowadays generally represented qualitatively as is shown for instance by Figs. 4 (c) and 8 (e). Around a core of warm and dry air, sucked down like a funnel during the phase of cyclogenesis, but having no longer any specific motion, the cooler, rainy and cloudy air of the hurricane vortex revolves with an average speed of 30 to 50 m/sec and is driven upward within a rather narrow ($r \simeq 10$ to 30 km) ' ring of convection ' (Depperman 1946) of about 20 km width and an inner radius of, say, 10 km. The two air masses are separated by a boundary zone which sometimes has been taken for the tropopause itself, sucked down from its normal height to this very low level owing to the dynamic effect of the underlying violent cyclonic vortex. It is more plausible, though, that this boundary zone has been formed by a

* Riehl in his most important contribution in this field of research (published in 1950 and not available to me until after this article was written) attributes the formation of this initial Low to an upper divergence.

similar process within the tropospheric air itself. In any case, it is evident that the most violent and dangerous tropical hurricanes have a central calm area – the eye – and a core containing warm and relatively dry air.

An assumption often met with in earlier text-books, and repeated as late as 1948 by Gherzi, that the hurricanes should be shallow mechanisms since they cannot survive a passage over even a rather low obstacle (1 to 2 km height), must be due to a false conclusion. The statical equation shows that the great pressure difference between the hurricane centre and its surroundings ($\Delta p/p \simeq 5$ per cent) decreases only slowly with height, even when a warm core in the hurricane is assumed*. On the other hand the warm-air funnel should not be confused with the humid air which is rising in the interior of the hurricane but outside this funnel, and which is able to drive the whole mechanism because it is somewhat warmer than the air outside it (cf. § 7)†. The warm-air funnel mainly acts as a ' fixed wall ' in the mechanism of the hurricane.

It is well known that within the tropical hurricanes, and partly also within the cyclones of our latitudes, one may distinguish two kinematically different zones, an outer and an inner one. The hurricane of November 1934, treated by Depperman (1946), affords an example of this kind of vortex; see the meteorograms (loc. cit., Fig. 1).

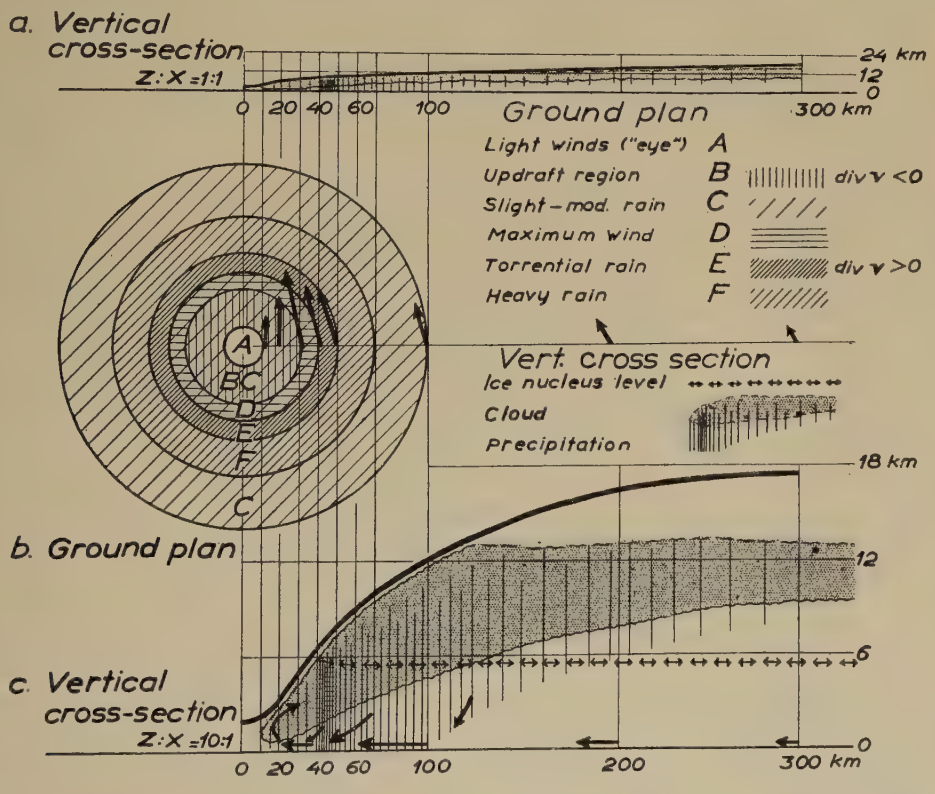


Figure 4. Schematic picture of the structure of the most violent Philippine hurricanes, the dimensions being mainly based on data taken from Depperman's paper of 1939. The vertical cross-sections only show one-half of the hurricane. The arrows in (b) represent the wind at different distances from the hurricane centre, their length being proportional to the wind velocity. It is seen that the zone of up-draught and the zone of maximum wind lie inside the zone of maximum rain (the heavily shaded area).

* Cf. Haurwitz (1935, pp. 45-49), who also calculates the height and shape of the warm-air funnel in the hurricane.

† In 1950 Arakawa arrived at exactly the opposite result when analysing a time section of the typhoon ' Kitty.' A re-analysis of his data made by the present writer, however, confirms the convective theory, Arakawa's discordant result being partly due to the above-mentioned confusion; the same applies to a time section of the Gulf hurricane 6 to 9 Oct. 1946 by Simpson 1947.

For the *outer zone*, we may assume $v_1 r$ = constant, and a constant angle of indraught α between the isobars and the wind. In that case the inward component of motion is inversely proportional to r, and we get $\mathrm{div}_2 \mathbf{v} = 0 = \frac{\partial w}{\partial z}$, i.e., the vertical component of motion w may remain nearly zero.

Since the above type of motion would imply infinite values of velocity and of convergence at the centre of the vortex, the motion takes another shape in its interior. For the sake of simplicity one often assumes that constant vorticity prevails within the *inner zone* of a hurricane (v_2/r = constant), and the angular momentum is then decreasing rapidly towards the centre. Assuming, once more, that the angle of inflow α is constant, or even decreasing inward, means that $\mathrm{div}_2 \mathbf{v} \ll 0$, and w must then increase with height. But already at a height of about 1 to 3 km above sea level, the current should be almost parallel to the isobars, and still higher up there will be an outflow from the centre. In this connection it may be pointed out that the streamlines in Figs. 5-8 of Thomson's paper on typhoons (1951) show a marked convergence around the typhoon centre even at the 3-km level; a closer inspection of the upper winds given in the figures, however, does not give much support for the construction used. As far as observations go, the wind field in Thomson's cases might have been non-divergent, i.e., purely rotational, within a circle of at least 100 km radius around the typhoon centre at the 3-km level.

The angle of inflow α within the friction layer is small at high latitudes; (over the sea it may be zero in certain parts of the wind field). Near the equator on the other hand, thanks to the smaller Coriolis force, α may be considerable (a generally accepted value for tropical hurricanes over sea being 30°; cf. Thomson 1951, p. 282). This means that the inflowing air of a tropical Low reaches its central part more rapidly than in our cyclones and has, therefore, conserved more of its angular momentum. This condition was in fact satisfied in constructing Fig. 4, where the velocity at a distance of 200 km from the centre is assumed to be 14 m/sec, at 100 km at least 24 m/sec, and at 40 km from the centre around 40 m/sec (or full hurricane force). At a distance of about 25 km from the centre, where the wind velocity is even greater, the centrifugal (and Coriolis) force is assumed to balance the horizontal gradient force. The air will then be forced upward, and above the frictional layer even outward along the sides of the tropopause funnel, which must be assumed to be comparatively impenetrable*. Haurwitz (1936a) found by theoretical considerations that in a hurricane, within the tropical region, α would have a value of about 36° at 200 km distance from the centre, but at a distance of 40 km from the centre would already be only 17°; and in one case he could confirm his calculations by observations. He claims that these circumstances show how the hurricane eye is formed in the most violent, i.e., the most typical, hurricanes.

It is then interesting to observe that some of Depperman's data show a marked zone of maximum wind just inside the heaviest rain, i.e. 30-40 km from the centre. According to these data (1946) the time interval between two wind maxima is sometimes only around 2 hr. This reduces the radius of the ring of maximum wind to less than 30 km. Inside the 10-km radius there is a rather weak wind, and inside the 5 km radius practically a calm. As already stated, these data are valid for the most violent kind of Philippines hurricanes. In the less violent hurricanes the radii just mentioned are at least twice as big. These circumstances seem to indicate that, the larger the amount of lability energy, the closer to the centre of the hurricane vortex the air will be drawn. Consequently, the smaller the hurricanes, the more intense they will be.

Before leaving Fig. 4, let us consider the distribution of precipitation in this kind of hurricane. The values are taken from 5 typical Philippine hurricanes, (1) where sufficiently

* The dynamics of this motion has now been rationally treated by Kleinschmidt (1951).

detailed records are available, (2) showing a big pressure-fall, and (3) being rather symmetrical. The outer limit of the precipitation zone in these cases has an average radius of about 100 km. At this distance the rain from the big cloud-shield begins to reach the earth's surface, attaining its greatest intensity already outside the zone of maximum wind. Inside this zone, within the zone of updraught, there is only comparatively slight rain.

The above rain distribution is explained by the vertical cross-section in Fig. 4 (c). At a distance of about 40 km from the centre the ice-nucleus level will on an average intersect the tropopause funnel and the upper limit of the main body of the updraught cloud-mass. Inside a circle through this point, therefore, the most effective factor for precipitation release is lacking. Moreover, the vertical depth of the cloud mass is here decreasing rapidly towards the centre of the hurricane. Therefore, even if the release of precipitation in tropical clouds is independent of ice-nuclei (Ludlam 1951), because of the small vertical extent of the cloud, only relatively slight rain, or no rain at all, will fall inside this limit – a circumstance presumably of vital importance for the hurricane mechanism.

6. Maintenance of the hurricane circulation

As a summary we may now state that the tropical hurricane, like the thundercloud, is a rain-machine that is mainly driven by the energy of overturning (' lability energy '). In the tropical hurricane, as distinguished from the cumulonimbus, the Coriolis and frictional forces, and evidently also the centrifugal force, constrain the air to a path such that it is finally lifted in an orderly fashion nearly simultaneously within a big

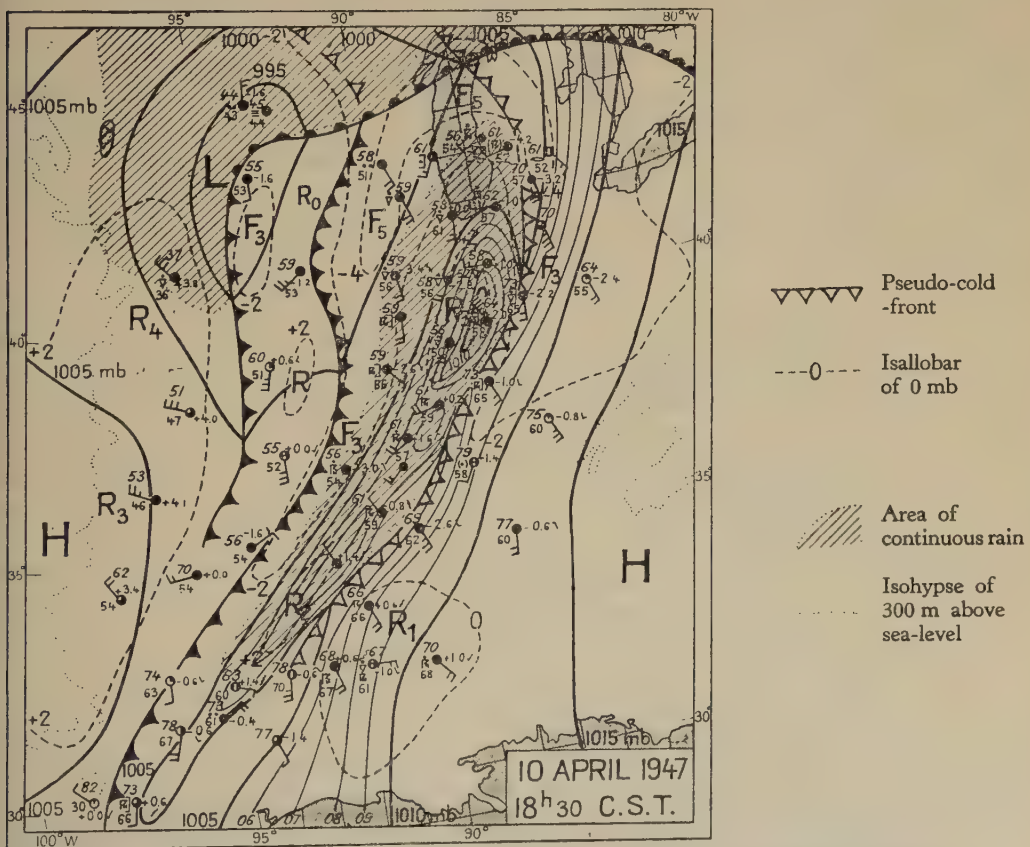


Figure 5. The pseudo-cold-front over the Middle West, U.S.A., 10 April 1947, 1830 C.S.T.

area of a ringlike shape, instead of being lifted in a disorderly fashion within rather small individual thunderclouds. In the first case the kinetic energy, as claimed already in 1928-1930 by Refsdal and myself, mainly takes the form of a great and violent vortex continually revolving in the horizontal plane (instead of in the shape of many small and short-lived circulations in a vertical plane, as in the thunderclouds).

This interpretation of the hurricane process meets, at any rate in part, Hann's objection to the convective hurricane theory, namely, that since a thundercloud does not give any appreciable pressure-fall but generally even a pressure-rise, it would be unreasonable to assume that a magnifying of this process would cause the strongest pressure-falls known. Refsdal has specially emphasized the fact that, since the horizontal extent of the thundercloud is only a few kilometres, neither the Coriolis force nor the centrifugal force (except within tornadoes) can exert much influence on the horizontal pressure field in this case.

On European weather-maps one may sometimes observe the indication of a convective cloud-system, 50-200 km in width, which gives strong rain associated with pressure-rises. This phenomenon has hitherto mostly been regarded as produced by a cold front, and the pressure rise was generally explained either by the invasion of cold air behind the front, or by the vertical acceleration within the pronounced convective cells.

In the U.S.A. these phenomena are even more marked. Fig. 5 presents a case with a rain-area apparently originating from the fusion of numerous separate thunderclouds (cf. the conditions in southern Texas). This mechanism deserves the name *pseudo-cold-front*: it is evidently formed within an originally homogeneous southerly current of Tropical air, without any invasion of true Polar air in the lower layers, since the dew point is only slightly lower to the west of the pseudo-front than to the east of it. In the rain area, which was between 200 and 300 km wide – with a rain intensity at the eastern border of about 1 in./hr, i.e., equalling that of a tropical hurricane – and which moved across the Middle and Eastern United States from west to east during 48 hr, there was all the time a considerable pressure rise (3 to 5 mb/3 hr). In fact, a small High of about 1010 mb is visible at R_3, within the rain area on Fig. 5*. The case of 23 August 1950, investigated by Delver (1951), shows an example of a typical pseudo-cold-front over

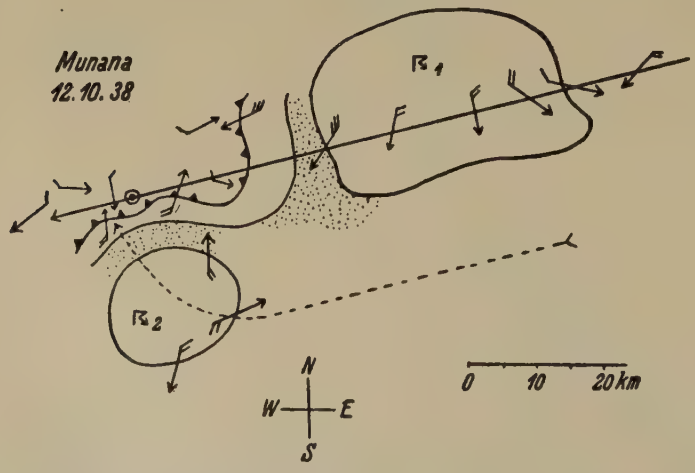


Figure 6. Field of temperature, wind and rainfall at the ground in connection with two thunderstorm cells, 1 and 2, at Munana (Angola, Africa), 12 Oct. 1938, at 15 M.E.T.; from G. A. Suckstorff (1939). Stippled area denotes rain. The front line shows the limit of the cooled air, clearly preceding the rain.

* All the synoptic maps showing this phenomenon were plotted and analysed during my stay at the Dept. of Meteorology of New York University in April 1947.

Holland with an accompanying small High, both of which are produced in connection with intense rain from a convective system, apparently formed the same day within Tropical air.

Such Highs are mainly caused by the cooling effect of the rain, as shown in several papers by Suckstorff (1935, 1936, 1939) and by Thorkelsson (1946). These works treat the thermodynamics of the moist-adiabatic descent of the air in heavy showers, thereby also explaining the rain-cooling quantitatively. Suckstorff's empirical results were attained by micro-registrations of air pressure, air temperature, wind and precipitation below typical thunderclouds with rain in Europe and in the Tropics. Fig. 6 shows one of Suckstorff's reconstructions of the ground plan of a tropical cumulonimbus. Fig. 7 represents a generalization which he had made from several individual maps of the type shown in Fig. 6, and also directly from his records and eye observations. It shows clearly that Suckstorff had realized that every fully-developed thundercloud, or convective cell, is accompanied by a small high-pressure region formed by rain-cooled air, and moving along with the thundercloud.

The above findings were thus published at least 10 years before the similar results attained by the U.S. Thunderstorm Project (cf. Byers and Braham (1949) in *The Thunderstorm*). They also confirmed the general conception of the Cb-model put forth by C. F. Brooks as early as 1922.

In the case of Fig. 5 the rain-cooling and the accompanying High were restricted to rather low layers of the air. Already at 3 km above sea level very little could be seen of these phenomena. Nevertheless they may become important, as we shall soon see when treating the formation of hurricanes.

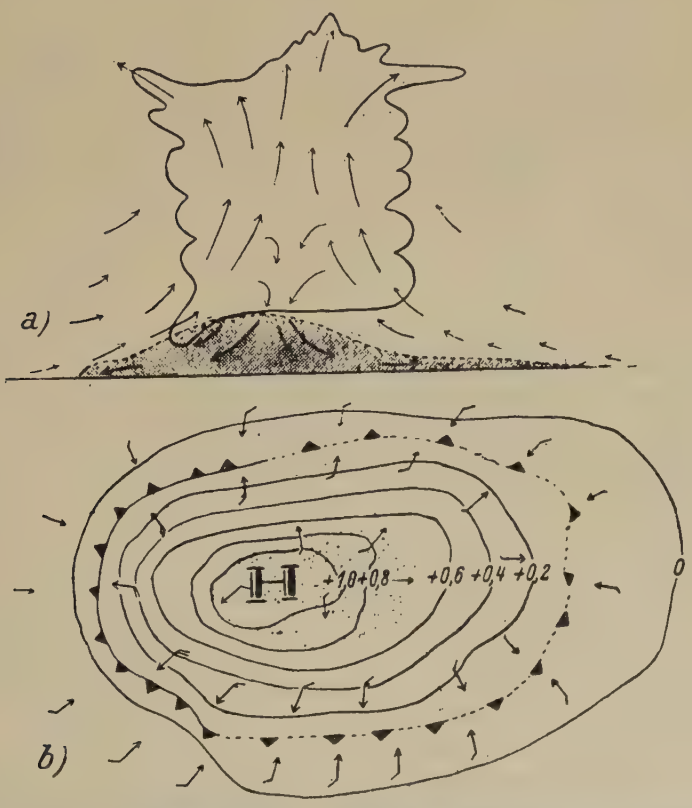


Figure 7. Cold-air High accompanying a mature cumulonimbus (from Suckstorff 1939). (a) Vertical section; shaded area shows dome of cold air. (b) Ground plan, cf. Fig. 6.

The strong divergence S. of Chicago in Fig. 5 is evidently connected with the rainfall-wind diverging from the precipitation area, especially where the rain is heavy, i.e., along its eastern edge (cf. Fig. 8, Section g). The sinking velocity in the case of Fig. 5 may be calculated from the divergence at the zero-level (about $3 \cdot 10^{-4}$ sec^{-1}), assuming that the divergence is mainly restricted to lower layers and decreases exponentially within these layers, having at 1 km a value = 1/e of the zero-level value. It is found to be around 0·3 m/sec at the 3-km level, thus being of the same magnitude as the average updraught velocity in a hurricane, cf. p. 152.

In the case of Fig. 5 the horizontal flow caused by the downdraught has such a great horizontal extent and duration that the Coriolis force will also contribute notably in determining the horizontal-pressure distribution. As already shown, the flow within such a convective system leads to the formation of an anticyclonic vortex around a pronounced High (instead of a cyclonic vortex with a Low at the centre). In the case of such a High it is thus impossible by conservation of angular momentum to arrive at an increase of the rotation around a centre.

Evidently, we encounter here one important reason why tropical hurricanes never form directly from convective cells, however vast and intense these may become, within a more or less homogeneous field of translation, not even over the sea near the doldrums in tropical air with a maximum of lability energy. Another circumstance, apparently of vital importance, the entrainment, is treated in § 8.

Fujiwhara in 1923 advanced a theory of tropical cyclogenesis based on the fusion of several very small vortices (and Lows) into a bigger one of great intensity. Now, from the above facts and reasoning it follows that these very small and rather feeble vortices will generally be quenched by their own precipitation as long as they form within a homogeneous current without an extensive and sufficiently strong steering dynamic factor such as a pre-existing Low of several hundred km horizontal extent.

The above-mentioned facts have remained enigmatic even to those who otherwise adhered in principle to the convective hurricane theory. Fig. 8, containing vertical sections of the main convective rain mechanisms, may now throw some light on this circumstance.

Sections a-c of Fig. 8 show the different stages of development of simple convective cells. Under certain circumstances, an individual cell may develop into a giant cumulonimbus. This cloud and rain mechanism will then degenerate within its interior. Assuming a symmetry of development which can never occur in reality, the final stage would then, as shown in Section d, represent in a way a tropical hurricane ' turned inside out,' or a ring-shaped pseudo-cold-front.

In Fig. 8, Section f, we have assumed that a small part of the hurricane ' torus ' has been detached from its general connection with the hurricane. The detached part thereby loses its steering – previously produced by the very pronounced low-pressure area and the intense cyclonic vortex. Consequently, at least over land, this mechanism will degenerate rapidly into a circulation of the thunder-cloud type, cf. Fig. 8, Section g, producing a small High instead of the original Low. When a tropical hurricane is forced to pass a mountainous region it will probably break into mechanisms of the types f-g, and this may explain why the hurricane vortex is then degenerating so rapidly.

Within very humid and labile air, on the other hand, the cloud development may lead to the fusion of several convective cells into one convective system : Fig. 8, Section h. The advancing border of this system will constitute a pseudo-cold-front. Thus, Fig. 8, Section h, also represents a schematic cross-section through such a weather system as the one over U.S.A. on 10 April 1947, shown on the map, Fig. 5.

Section e of Fig. 8 shows a typical hurricane. Remembering that the heaviest rain presumably falls in a ring-shaped area outside the updraught zone, the rain-cooling and

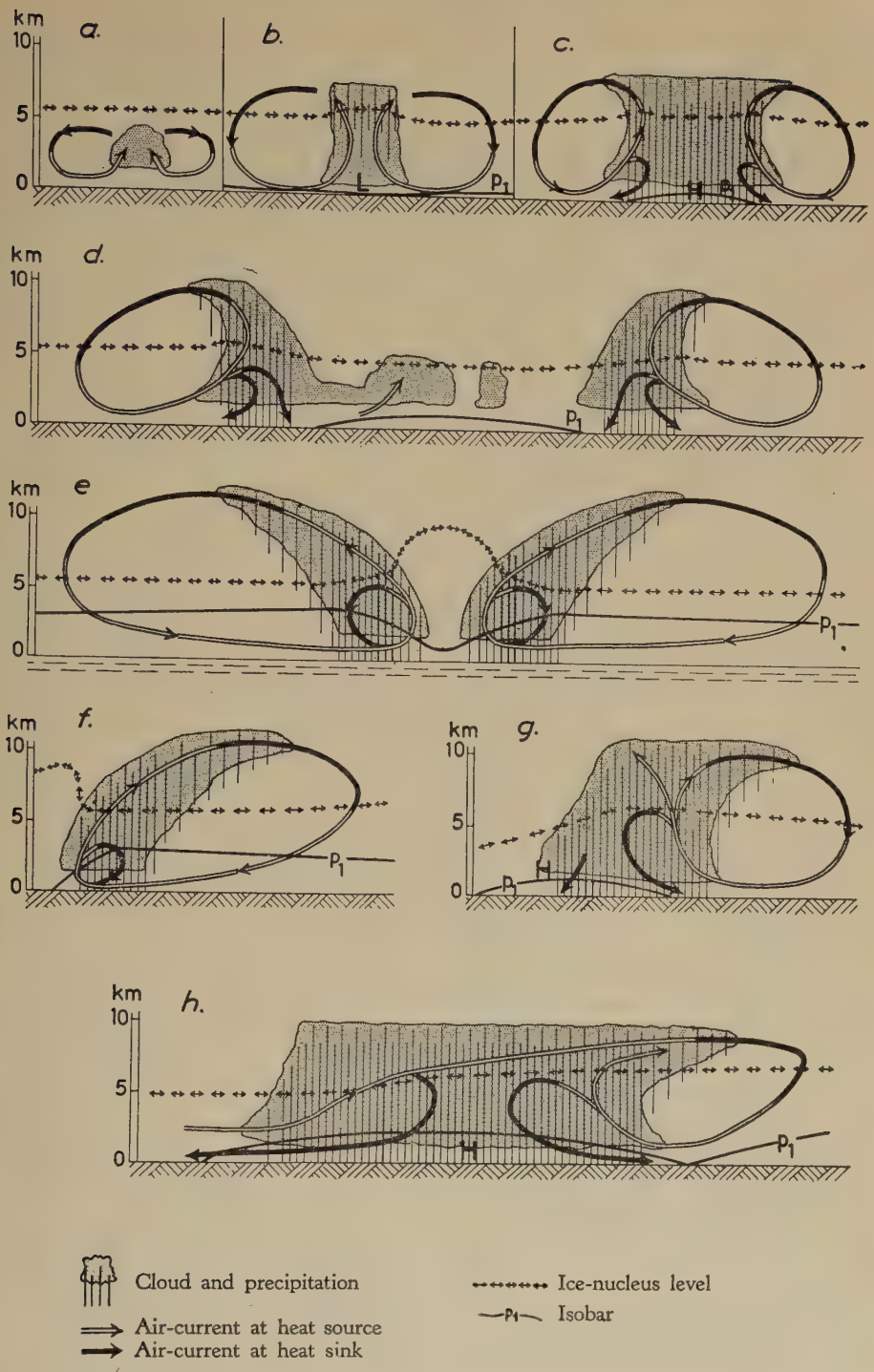
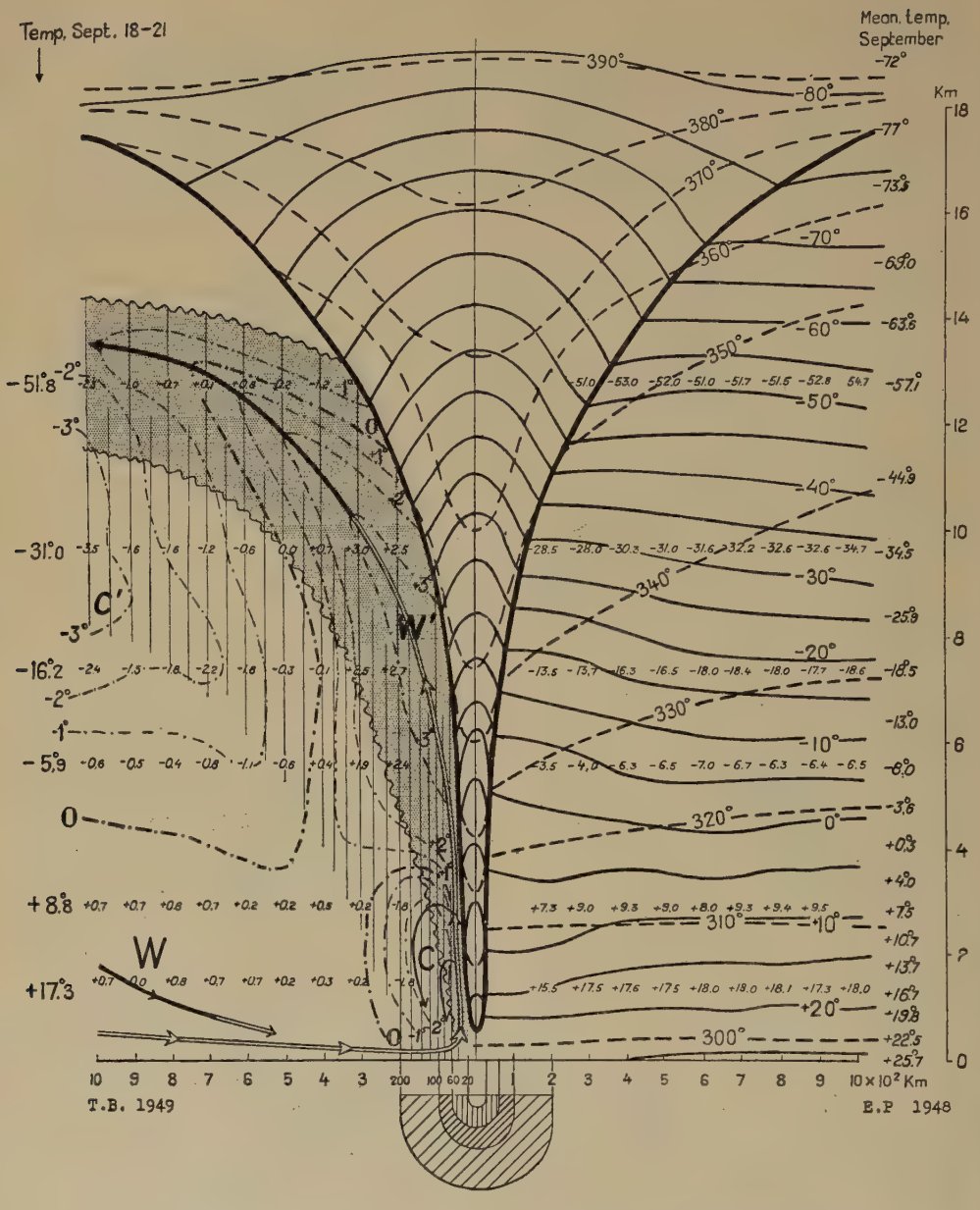


Figure 8. Main types of rain-producing convective cells and systems. (Schematic vertical cross-sections). In Sections c, d, g, h the H denotes the small and feeble High that develops at lower levels within the region of rain-cooling and rainfall-wind. Sections : a. Cu congestus, b. Cb calvus, c. Cb capillatus, d. ring-shaped pseudo-cold-front with central High, e. tropical hurricane with central Low, f. fragment of tropical hurricane, g. asymmetric Cb, h. convective system (in U.S.A., 'squall-line') with post-frontal High.



In left part of vertical cross-section

-1- Air 1°C cooler than the average for this level

Precipitation \ Cloud

Air-current at heat source

Air-current at heat sink

In central and right part of vertical cross-section and in ground plan

Main updraught region

Heavy rain

Light to moderate rain

Isotherm °C

Isentropic line °C

Figure 9. Vertical cross-section of the Miami-New Orleans hurricane 18-21 Sept. 1947, and horizontal projection of its central region (below). Right-half of figure from Palmén (1948).

the rainfall-wind may now be fitted into a relatively small direct circulation wheel O_I (in vertical planes) in the interior of the hurricane. Here the cool rainfall-wind is rapidly warmed again over the sea surface (and later, above the condensation level, in the lower part of the hurricane cloud, by the latent heat). In this way the opposing action of the rainfall-wind will be transformed into a supporting action.

The greatest amount of energy, though, is no doubt provided by a large outer circulation wheel O_E. Within this wheel the air is cooled by radiation in the altostratus and cirrostratus part of the cloud (and by the evaporation of this cloud and its precipitation), and even more at the levels 8 to 12 km in the clear air at a greater distance from the centre. The air is then heated again in the same way as in the smaller wheel O_I. See also § 9.

7. Palmén's proof of the direct circulation in hurricanes

The case of 17 to 21 Sept. 1947

On 17 to 21 Sept. 1947 the south of the U.S.A. was struck by a hurricane which passed precisely over the two cities in U.S.A. that have a special Hurricane Weather Service : Miami and New Orleans. It moved for several days at low latitudes without degenerating and mainly over sea, but nevertheless keeping in touch with the good network of radiosonde stations within the U.S.A.

Palmén has utilized this rare and lucky circumstance for demonstrating the structure of a true tropical hurricane. Even in this case, however, it was necessary to use a statistical synoptic method which could only supply an average temperature within the hurricane, at different levels and distances from the centre, during the four days 18 to 21 Sept. 1947. Palmén's results are represented in the right-half of the vertical cross-section through the hurricane, Fig. 9. The diagram provides the expected confirmation of the convective theory : a tropical hurricane, as long as it lies over warm sea and is not filling, is driven according to the convective scheme, by rising air which remains warmer than the ambient air up to great heights. The temperature-entropy solenoids in the right-half of the picture show, in fact, that the air motion in this hurricane (see the arrows) corresponds to a direct circulation – (Riehl 1950 makes a postulate to the same effect, whereas Arakawa 1950 claims to have proved the opposite, cf. footnote on p. 139).

In the left-half of the diagram, Fig. 9, as a complement to Palmén's investigation, I have added the mean temperatures for the same hurricane at different levels, and the deviations from these means at the different measuring points of each level. There are two regions where the air is markedly warmer than the average (W and W′), and two regions where it is colder (C and C′). As a result of this temperature distribution, and based on what is said above of the general structure of the tropical hurricane, it was then also possible, in the same part of the figure, to indicate the zone of cloud and precipitation of the hurricane. As seen from the figure, the upper warm region W′ can be interpreted as conditioned by the rising warm air within which the latent heat is released. Within the region C′ the air is cooled by radiation, and also by the evaporation of the upper cloud-shield and its precipitation. The lower, less marked, warm area W indicates the warming from the sea surface transmitted by the turbulence. Within the region C the air is cooled by the intense precipitation.

8. Distribution of rainfall and updraught in hurricanes

Thanks to the hourly measurements at the rainfall stations of the U.S. Weather Bureau, it has been possible to reconstruct the distribution of rain intensity within the above-mentioned hurricane, as it crossed Florida on the 17th. In order to arrive at a conclusive result concerning this kind of rain mechanism, a special technique of analysis

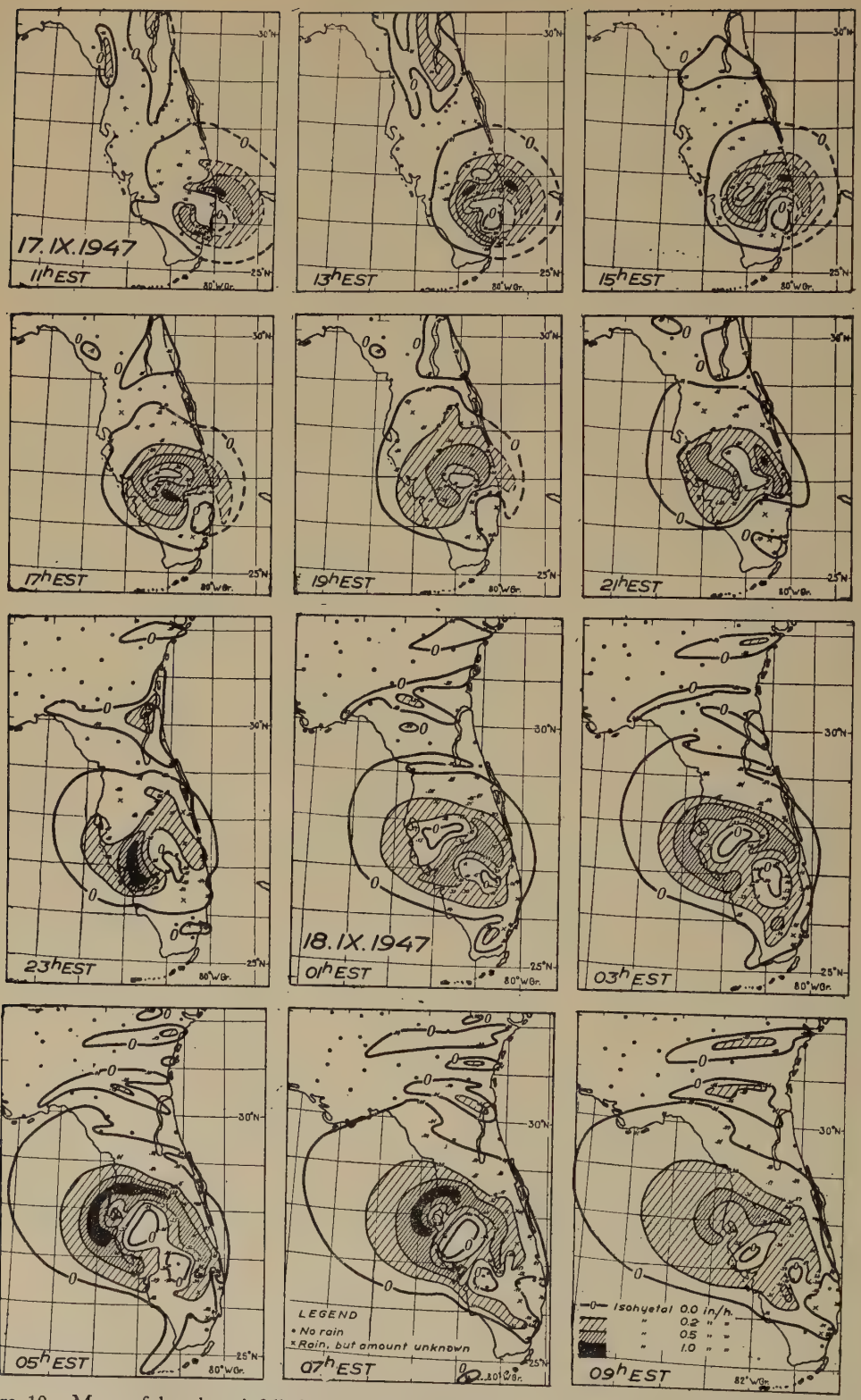


Figure 10. Maps of hourly rainfall from the major hurricane of Sept. 1947 when crossing Florida. (In Figs. 10, 11 and 12 a dot, •, denotes ' no rain ').

had to be developed : a method based on successive and repeated mutual adjustments of consecutive maps to achieve an optimum kinematic consistency of the motion and development of the mechanism studied.

Almost every hourly map in Fig. 10 shows a ring-shaped or ' hollow ' rain area, with a region of slight or even no rain at the centre. The zone of maximum precipitation presents an intensity between 0·5 and 1 in./hr. As seen from Fig. 10, the intensity was not evenly distributed around the centre, its maximum shifting rapidly to different sectors as the centre passed over Florida.

Assuming that the inner, rainless region coincides with the storm centre, the ' eye,' we get a method of mapping and tracing the track of a hurricane, more detailed, and presumably more reliable, than that of an ordinary synoptic map, which is based on a very scarce network of stations, some of which often are put out of action just at the critical moment, (cf. also p. 136). There is, for instance, a great discrepancy between the hurricane track treated here as given by Palmén (deduced from the official synoptic reports, where the station Fort Myers was missing at the very passage of the storm) and the track obtained from the rainfall maps. In fact, the latter lies 100 or 200 km farther north as the hurricane crosses Florida.

It has also been possible to reconstruct the total precipitation of the same hurricane during the four days when it passed from Florida over the Gulf and up through the Mississippi valley. Interpolation between Florida and the Mississippi valley gives the rain distribution of Fig. 11, which clearly shows that the ' trace ' left by the ring-shaped

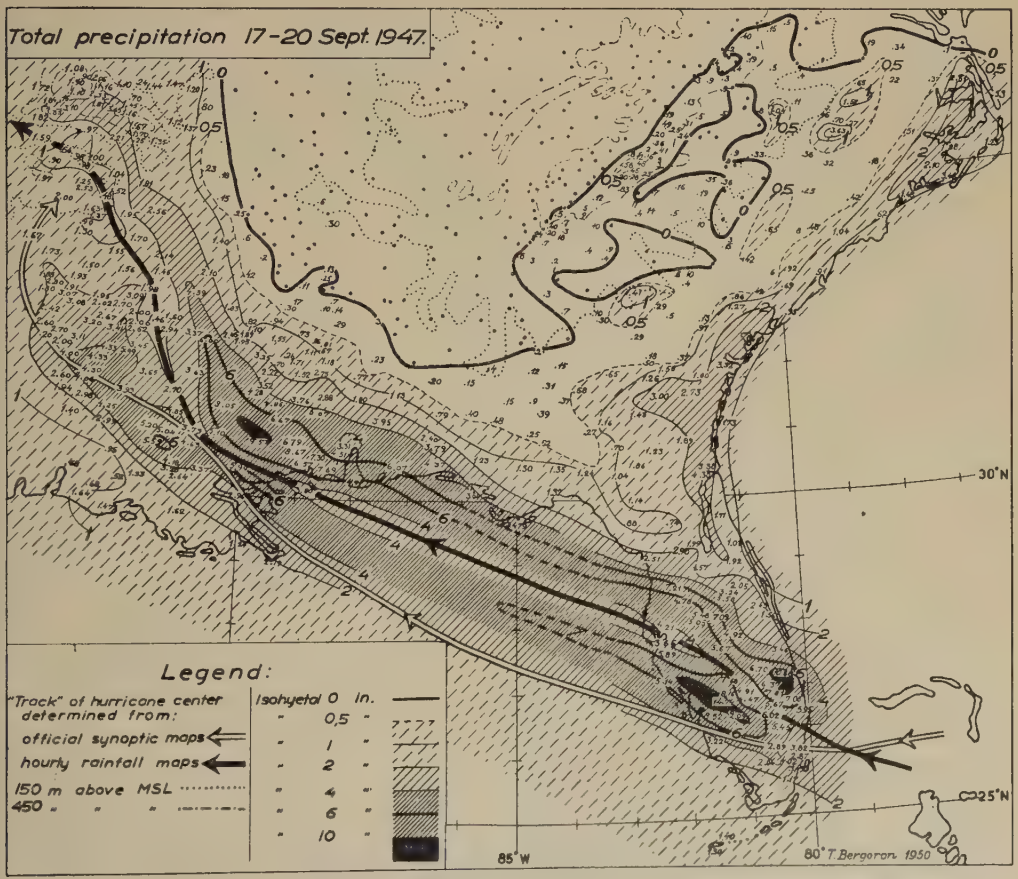
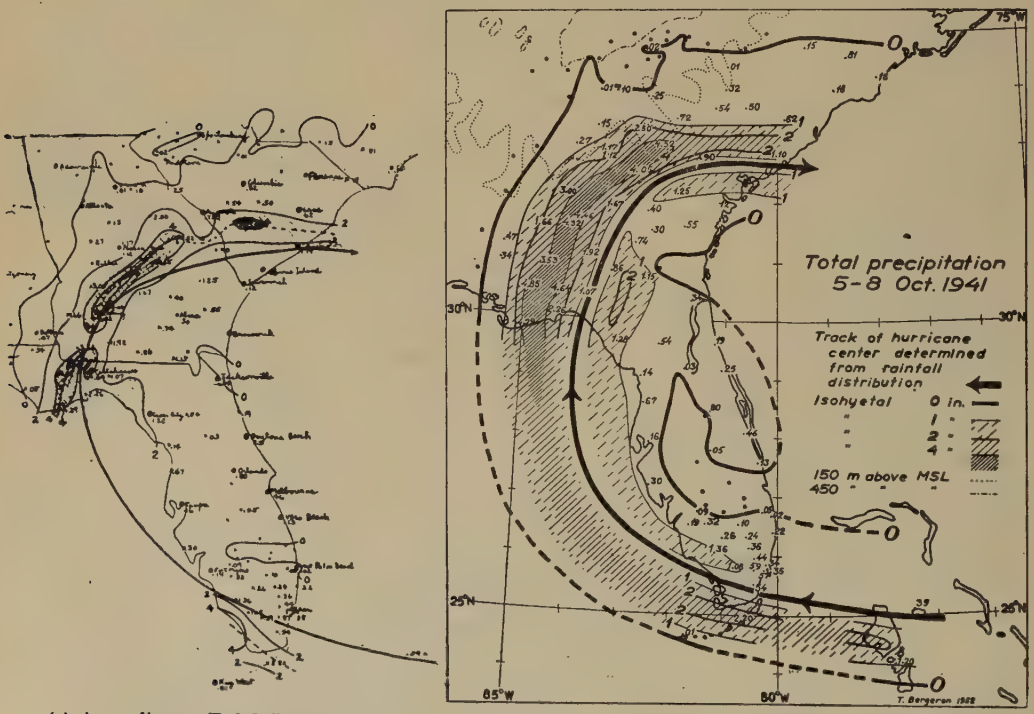


Figure 11. Total rainfall in four days from the major U.S. hurricane of Sept. 1947.

rain area, as one would expect, consists of two parallel rain bands, each with a maximum rainfall of 5 to 10 in., separated by a region with less than 3 in. This gives another proof of the general structure of the precipitation area in this kind of a hurricane and of the hurricane itself.

Fig. 11 also confirms the assumption that this hurricane must have passed inland, from the Gulf into the Mississippi region, along a more northerly track than has been assumed hitherto. The discrepancy may partly be explained by an observation made on my sequence of 96 separate hourly rainfall maps, viz., that a hurricane seemingly propagates by steps in such a fashion that a secondary centre, at the periphery of the main one, is gradually taking over the role of the main centre, while the old centre is decaying. This process may also explain certain irregularities in the development and propagation of tropical hurricanes reported and discussed by several observers and writers in earlier and in recent cases*.

E. M. Brooks has published a rainfall map, similar to that of Fig. 11, for the hurricane of 5-8 October 1941, which crossed southern Florida, then went NNW over the Gulf and reached south Georgia. E. M. Brooks' map, Fig. 12a, does not show any real signs of a double rain-area, but it should be noted that the same rainfall data can be interpreted in another way, as shown in Fig. 12b. From this figure we may surmise that the track of the main centre passed more to the east, over the Apalachee Bay and N. Florida.



(a) According to E. M. Brooks (1946). (b) Re-analysis made for the present paper.

Figure 12. Total rainfall from the Florida hurricane of 5 to 8 Oct. 1941, and its path.

* Apart from Fujiwhara's theory of multiple centres during the stage of typhoon formation (discussed on p. 144 above) and different reports of multiple hurricane eyes or lulls with several hours' interval (cf. Sumner 1948), attention is drawn to the theory of Yeh (1950) on the oscillating motion of Atlantic hurricanes and to the oscillating hurricane tracks drawn by Horn 1951 and discussed by Cressman 1951. It seems obvious, though, to the present writer, that the scanty network of observations available over sea, including the reconnaissance flights, can merely suggest such oscillations, but not prove their existence; the proof could only be rendered by simultaneous observations from a network of stations at fixed points even denser than the one used in Fig. 10 and observing all meteorological elements.

Moreover, at least in Georgia, there is an indication of a double rain area, which probably could have been better traced if the network of rain stations had been a closer one.

In a model hurricane we may now assume a component of frictional inflow at the surface $v_{i0} = 7$ m/sec at 200 km from the centre (corresponding to $v = 14$ m/sec and $\alpha = 30°$, cf. § 5), and a decrease of the inflow with height analogous to the one postulated for the divergence on p. 144. Making plausible assumptions concerning the moisture content of the inflowing air (20 g/m^3 as an average value for the whole layer of inflow, corresponding roughly to the state at the 900 m level) we get a total amount of water vapour passing through the storm $m_w \simeq 1{\cdot}8 \times 10^5$ ton/sec $\simeq 6{\cdot}4 \times 10^8$ ton/hr or $15{\cdot}5 \times 10^9$ ton/d.

Now, the Florida hurricane of Sept. 1946, studied by Longley (1948), gave $m'_R \simeq 7{\cdot}3 \times 10^9$ ton rain per day. The major Florida hurricane of September 1947, according to Fig. 11 during the 17th Sept. gave $m''_R \simeq 5 \times 10^8$ ton rain an hour, or 12×10^9 ton/d.

We may assume that the general conditions in these two hurricanes did not deviate very much from those of the above model. The quotients $q' = m'_R/m_w = 0{\cdot}5$ and $q'' = m''_R/m_w = 0{\cdot}8$ indicate, as one would expect, that only a fraction of the water vapour passing through these hurricane mechanisms reached the earth as precipitation within the storm in question. The following factors may have contributed to this result:

(1) There might be a considerable entrainment, i.e., a turbulent exchange between the column of rising moist air at the storm centre and the dry ambient air at higher levels. In a hurricane, though, the updraught air will be to a great extent sheltered from mixing with dry air because of two reasons. On the outer side of the hurricane cloud-system, the falling precipitation is moistening the air and thereby to a certain extent protecting the updraught air from direct contact with any dry air. Inside the cloud system the temperature inversion at the transition to the warm-air funnel will have a similar effect.

Consequently, the influence of entrainment will presumably be small, and the main precipitation deficit will be due to:

(2) the evaporation, just mentioned, of the precipitation from the hurricane cloud shield before it reaches the ground,

(3) evaporation of the cloud shield itself within the sinking air at the outskirts of the storm area and

(4) the uncondensed moisture of the air diverging from the hurricane area at high levels.

(5) Inside the circle of 200-km radius, on the other hand, O_I will provide a considerable contribution to the amount of air and moisture flowing through the hurricane, beyond the amount that is controlled by the above inflow v_{i0} through a vertical cylinder of 200-km radius. In the outskirts of O_I the subsiding air may be rather dry, it is true; but when reaching the zone of updraught it will be almost saturated owing to the evaporation of precipitation and to turbulent transport of moisture from the sea surface. This circumstance may explain why q' and q'' were found after all to lie comparatively near the maximum value unity.

In order to estimate the whole of m_w one ought, presumably, to compute the inflow through a cylinder just outside the zone of updraught, although the values of α and v are here even more uncertain. Using a figure from Haurwitz (1936b), $\alpha = 17°$ at $r = 40$ km, and the value $v_{w0} = 40$ m/sec (from Fig. 4 and p. 140), we find $v_{i0} = 9$ m/sec. All other data and assumptions remaining the same, this would give $m_w = 20 \times 10^9$ ton/d and $q'_1 = 0{\cdot}37$, $q''_1 = 0{\cdot}48$.

Now, E. M. Brooks (1946) has arrived at analogous figures for the Florida hurricane of Oct. 1941 in a slightly different way, simply by using a value of the convergence $C_m = v_{i0}/r$

and assuming the amount of rainfall to depend on the difference between the mixing ratio of the air in the frictional indraught layer (about 20 g/kg) and at the upper level of general divergence (about 5 g/kg). Thereby, however, only losses of moisture of the type (4) and (3), in part, are taken into account. Brooks in this way finds values of the precipitation rate within the Florida hurricane of Oct. 1941 at different distances r from the centre that seem to coincide well with observation when $100 \text{ km} < r < 135 \text{ km}$, but give much too great values at shorter distances.

The general explanation of this discrepancy is hinted at above. The reason why the difference is not uniform lies in the fact that the local convergence does not correspond to the local precipitation rate since the rising current is not vertical, and since the precipitation release, according to § 5, is less efficient within the region of maximum convergence and updraught, cf. Fig. 9.

An overall value $\bar{w}$ of the updraught in the model hurricane, inside the 140-km radius, would be about 0·15 m/sec, i.e., even less than the absolute value of the overall downdraught within a rain area of the same width at a pseudo-cold-front of the type shown in Fig. 5. The convergence data of Brooks (1946) correspond to $\bar{w} = 0{\cdot}02$, 0·03 and 0·09 m/sec at $r = 135$, 100 and 50 km.

In order, however, to arrive at local figures for the updraught velocities w in hurricanes we need to consider the real size of the updraught area. We may assume most of the updraught to be concentrated between two concentric circles of 15 km and 30 km radius (cf. Depperman 1946). In that case we find, for the model hurricane, $w = 4$ m/sec, in good agreement with Depperman's value of 5 m/sec.

In the case of the pseudo-cold-front studied in Fig. 5 the overall downdraught $\bar{w}$ was $-0{\cdot}3$ m/sec, but if we assume w to be concentrated mainly in the region of strongest precipitation (only some 10 to 20 per cent of the whole area) the downdraught may locally have reached values of 1 to 5 m/sec, being then of the same order as the real hurricane updraught. Thus, even this purely kinematic circumstance indicates that the hurricane circulation O_I, being analogous to the circulation at a pseudo-cold-front, may be decisive for the hurricane process as a whole. When O_I is coupled parallel with O_E a hurricane vortex may be maintained; in the opposite case it may become impossible.

9. The problem of hurricane formation

During the hurricane season the Tropical fronts reach their highest latitudes, with a maximum Coriolis force. At least one of the two tropical air masses converging in these regions is generally a little cooler than the sea surface ('cold mass '), and in the doldrums the two air masses are subject to a vertical stretching. Both these effects, tending to increase the instability of the converging air, will be more pronounced in the western part of each ocean and specially within the deepest sections of the equatorial low-pressure trough. In these areas, therefore, the conditions are in some respects most favourable for the formation of tropical hurricanes. The main problem, though, is still unsolved : namely to explain how a large-scale cyclonic circulation with a continuous area of heavy rain is started without (partly) quenching itself at the start – at any rate in its lower parts – as is the case with the thundercloud (cf. the mechanism c, d and f-h in Fig. 8), and how it is intensified into a hurricane vortex.

Fig. 13 shows a possible process of the type sought for. It has been based on the pre-existence of a well-defined tropical Low. At or near the centre of this Low, both the instability of the air and the convergence, and thus also the impulse of lifting, will generally be at a maximum. Consequently, any formation of cumulonimbus clouds should start in this region, and in the early stages the Cb should attain their greatest development there; cf. Fig. 13 (A).

If this evolution could take place over *land*, without any external influences to disturb its radial symmetry, the final result would, as mentioned above, probably be a kind of ring-shaped pseudo-cold-front with a marked anticyclonic counter-circulation around a central High as in Fig. 8, d. In fact, the air in the friction layer would be cooled by the rain to a wet-bulb potential temperature θ_w decidedly below that of higher levels, and it would no longer be able to rise in the interior of the Low, in spite of the frictional inflow. Thus, the Low would rapidly be depleted and, maybe, even transformed into a shallow High in its interior (cf. § 11).

In the warm oceans, on the other hand, there is a practically inexhaustible supply of heat which, thanks to convection in the ocean water, may keep the surface temperature of the ocean quasi-constant. Consequently, over sea the air temperature may be kept at 27° to 28°C in the lowest layers of a hurricane, even within the rain area, and in spite of the expansion to which the air is subject when sucked into a Low. Under such conditions there will be no considerable ' expansion cooling,' no rain cooling within the turbulence layer and no pronounced central High – at most a slight or variable (thermal and frictional) downdraught spreading horizontally in the lowest layer from the region of maximum rain. Instead there will be a considerable increase of the wet-bulb potential temperature (entropy) of the indraught air, the greater the lower the pressure is at the centre. At a given moment this increase of θ_w will permit the air to rise to great heights subject to an intense updraught. Moreover, owing to the much lesser friction over sea, for a given energy production a more intense cyclonic circulation and a deeper Low may be maintained over sea than over land.

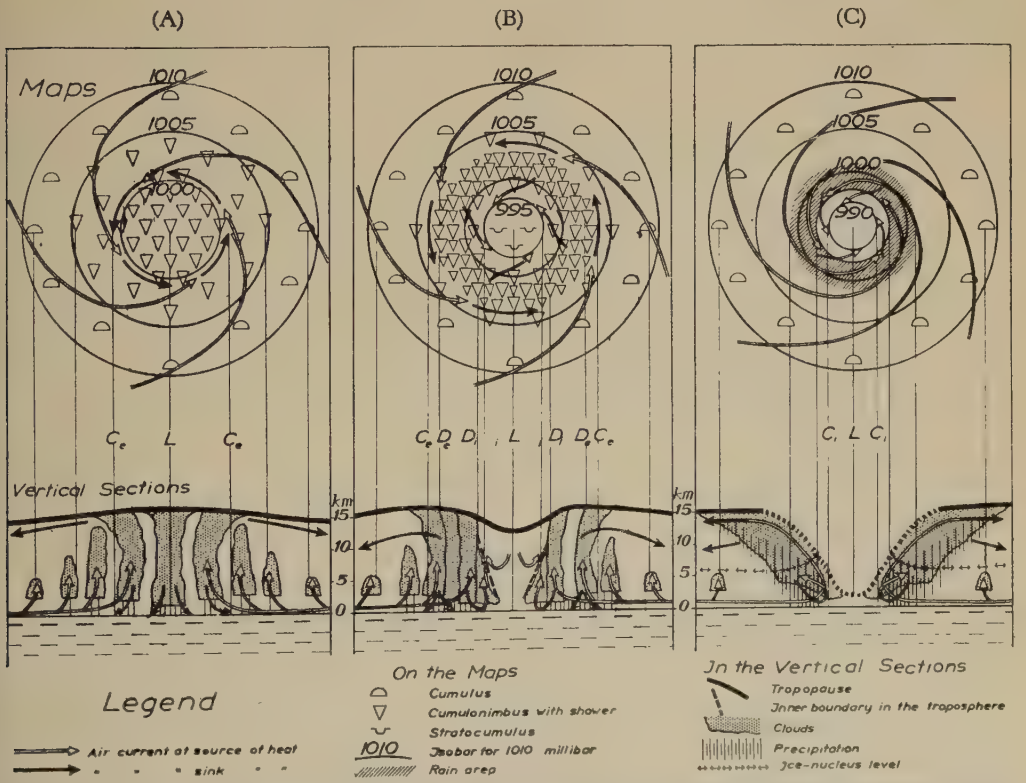


Figure 13. Hypothetical mechanism of hurricane formation. (A), symmetric tropical Low with maximum instability at centre; (B), same Low with ring of instability clouds; (C), the Low is ' inverted ' and intensified into a hurricane.

Therefore, as we shall see, over a *warm ocean* the ' inverting ' of the cyclone outlined below, and shown schematically in Fig. 13 (B)-(C), might possibly take place, while over land it is impossible.

Thanks to the downdraught within big cumulonimbus-clouds there may, even over sea, at first develop a feeble counter-circulation within the interior of the Low in its lower layers. The results will then be an outer zone of convergence C_e shown in Fig. 13 (A) i.e., a convergence between the frictional inflow from the periphery of the Low and the outflowing component of the rainfall-wind. A widening fetch of the inflow and a general deepening of the Low – perhaps in connection with an upper divergence of the kind described by Riehl 1950, and favoured by an increased formation of clouds – will increase the cyclonic vorticity, the pressure gradient, and the indraught within the friction layer. When surpassing a certain threshold value, the indraught of air from the surroundings of the disturbance, having a comparatively high cyclonic angular momentum, must break through the external part D_e of the divergence ring around the rainfall wind and join its internal branch D_i: the disturbance has become ' inverted.' At this stage the vorticity will increase violently in the lower layers near the storm centre, and this state is then rapidly propagated upward following the updraught. Thus, the intense vortex will extend also into higher layers, and the apparent tropopause is then sucked down as shown by Fig. 13, (B) and (C). The above reasoning is to a certain extent supported by the theory of hurricane formation expounded by Sawyer (1947, cf. especially p. 117 and Figs. 9, 11).

The subsidence of the central tropospheric air-column, together with the inflowing component D_i at low levels, may, already before this stage, have formed an inner boundary-zone even steeper than the sides of the tropopause funnel and finally becoming the real boundary of the ' warm-air ' core.

The above ' inverting ' process is obviously the critical phase of the cyclogenesis, and still difficult to explain satisfactorily*. Evidently, it is only successful on very few occasions, which must be one main reason why tropical hurricanes are so rare even within the regions delineated in Fig. 1. Two papers by Emon (1946, 1948), on ' abortive ' tropical hurricanes experienced in the Madagascar Weather Service, are interesting in this connection.

Assuming the ' inverting ' to be possible, the inverted state can only be maintained because the temperature of the warm air flowing into the Low, in the layers nearest to the ocean, is kept very near the temperature of the ocean in spite of the torrential cool rain and the expansion of the air; owing to the latter circumstance the entropy is even increasing and at last attains a very high value. Therefore, both O_I and O_E become direct circulations of considerable specific power, each being of the order of 10^4 erg cm^{-2} sec^{-1}, according to certain calculations to be published later. The transition from stage (B) to (C) is probably an abrupt one connected with a big increase of the rotational velocity and a final sucking down of the warm-air funnel. This, in its turn, explains the shape and mode of action of the final continuous cloud system and rain-area.

Fig. 14 is intended to show the presumptive functioning of the above-mentioned ' inverting process ' or cyclogenesis. In the upper part of the figure horizontal arrows represent the general indraught in lower layers; the curved arrows show the rainfall wind and the compensating updraught outside this region at stage I. The cloud system and the tropopause within one-half of the hurricane vortex are indicated in a first position I and in a later position II. The curves in the lower part of the figures illustrate plausible relative values of the vertical components (upward and downward) caused by the different

* It may be started, or at any rate be favoured, by the above-mentioned upper divergence postulated by Riehl (1950), or by the dynamically labile air-ring proposed by Kleinschmidt (1951, Fig. 1); cf. Sawyer (1947, Sect. 7-8).

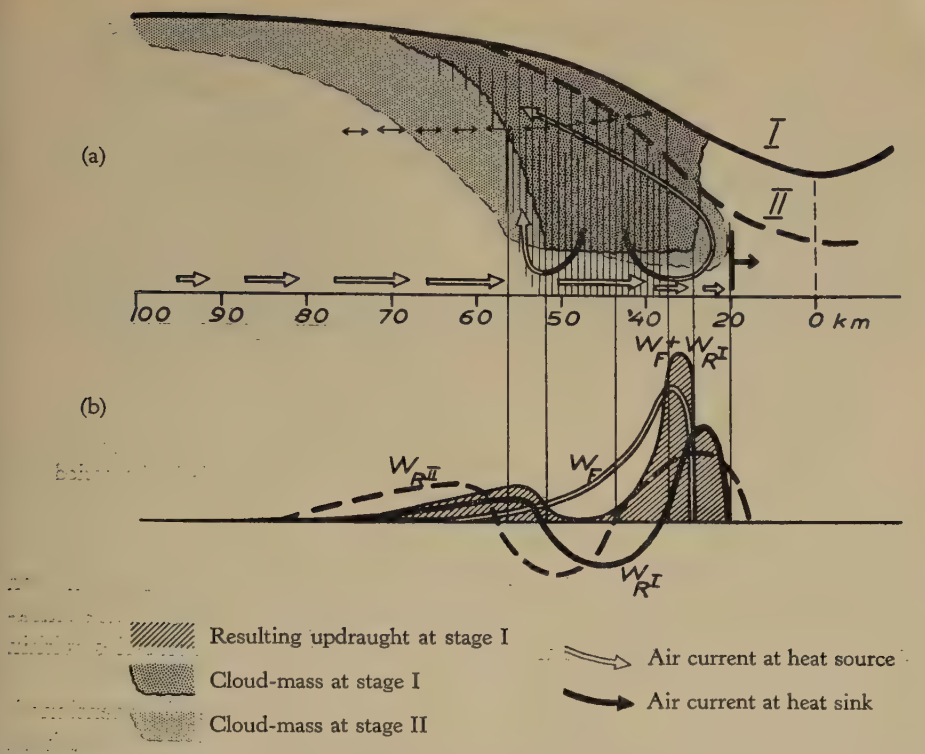


Figure 14. (a) Conjectured vertical cross-section of the cloud system of a tropical hurricane during its stage of intensification. (b) Diagram of plausible relative values of updraught and downdraught components in the above process.

factors mentioned: w_R rainfall wind, w_F updraught due to frictional convergence. The curve $w_F + w_R$, and the hatched region, show the sum of the two vertical wind components; in the figure this sum is assumed to be positive everywhere.

Naturally, there may be several other possibilities for explaining the formation of a hurricane. There will remain, though, as necessary conditions:

(1) a region with an evenly distributed and moderate surface friction where a considerable store of lability energy due to vertical instability can be maintained in the atmosphere in spite of the rain-cooling, i.e. a warm ocean surface,

(2) a sufficient but not too great Coriolis force within this region and

(3) a precursory stage that favours an orderly inflow and lifting of the air within an area of sufficient width.

10. Formation of extra-tropical summer hurricanes

Within the temperate zone, disturbances sometimes occur showing such a rare intensity and small horizontal extent that they may be regarded as a kind of extra-tropical hurricanes. These phenomena may be studied in more detail by utilizing the good network of ground stations, and of soundings, available on the continents of Europe and America; the results may then to a certain extent be applied also to the tropical hurricanes.

The season of occurrence of these extra-tropical hurricanes is generally summer or autumn. In the typical tropical hurricanes, as well as in the hurricanes of our latitudes, the distribution of cloud and temperature is a rather symmetric one. Because of this symmetry the assumption that the tropical hurricanes were caused by frontal disturbances

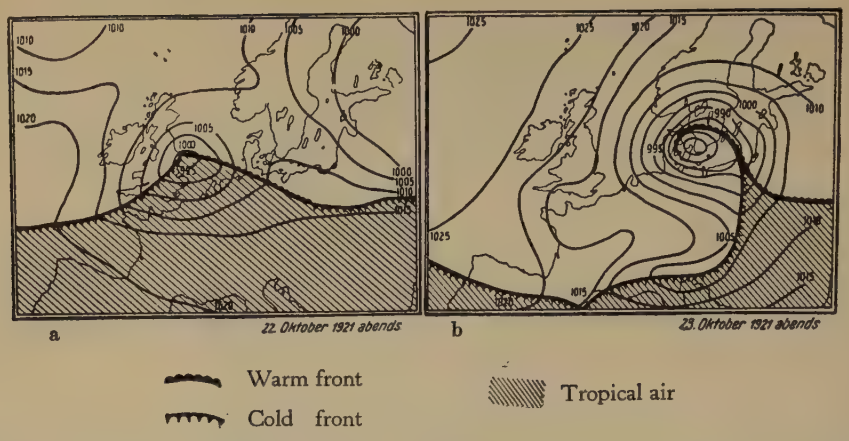


Figure 15. Development of an extra-tropical hurricane over southern Scandinavia in the period 22 to 23 Oct. 1921.

has met much opposition. As shown in § 9, they probably do not form directly from such disturbances. There is much evidence to support the assumption that both the extra-tropical summer hurricanes and the true tropical hurricanes form as secondaries within a Low that no longer contains any fronts.

Among typical extra-tropical hurricanes the following five cases may be mentioned.

Fig. 15 illustrates the famous cyclone which on the 23 Oct. 1921 reached southern Scandinavia, Holland and northern Germany causing wind forces 9 to 12 in this region. In the morning of the previous day a shallow Low was lying over England with moderate winds and a vast warm sector over south-western Europe. It could be expected to move eastward, occlude and cause a rather strong deepening of the Low. In such a case one may generally expect a pressure fall of about 1 mb/hr. The wind forces to be expected within the next 24 hr in connection with such a deepening would not exceed 8-9. In the evening of the 22nd the centre of the disturbance, which had deepened about 15 mb (or 1·4 mb/hr), was lying over the southern North Sea (Fig. 15a), and the wind had increased to 6 or 7, but there was nowhere a gale as yet. Nevertheless a supplementary warning for whole gale to storm was issued from Stockholm for the Swedish West Coast, whereas other countries reached later by the storm did not even issue a gale-warning. On Sunday morning, 23 Oct., the core of the storm was lying near Copenhagen with a pressure of about 970 mb at the centre (cf. Fig. 15b, this map differing only slightly from the morning map of that day). Over Denmark, Skagerack, Kattegatt, south-western Sweden, the southern Baltic and northern Germany there was now a storm to a hurricane. The increase of the wind force and the deepening of the Low had amounted to double the normal.

Up till now we have generally tried to explain this development by assuming that during the latter part of the night between 22 and 23 Oct. an orderly overturning of conditionally unstable air in the interior of the Low had taken place. The Low was at this time probably already occluded and filled all round with unstable maritime Polar air. Thanks to frictional inflow – at first only producing a slow upsliding motion at an angle of 1 : 100 above some still existing frontal surfaces – and the vertical stretching of the air thus affected, the vertical lapse rate and the statical instability were further increased. Moreover, the level of condensation was thereby lowered, and so the store of lability energy was increased still more (cf. Fig. 2). It was then assumed that suddenly an orderly and more or less vertical lifting of the air began at the core of the Low. This lifting should start as soon

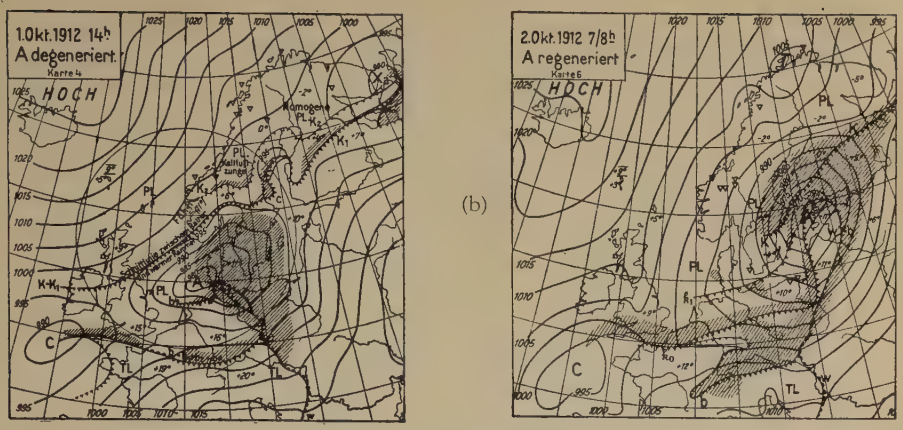


Figure 16. Development of an extra-tropical hurricane over the Baltic, 1 to 2 Oct. 1912, from Schröder (1929).

as the deepening of the centre, and thereby also the frictional inflow and the instability of the air mass, had passed a certain threshold value. Next morning the effect of this overturning was seen over Jutland where a full hurricane was raging. The rain-area over Denmark was much bigger and more intense than is usual within an occluded cyclone. In the above explanation, however, we had evidently not taken into account the cause and effect of this heavy rain.

An analogous case, occurring within the period of 30 Sept. to 2 Oct. 1912, was treated by Schröder in 1929. Since the time chosen coincided with some International aerological days, he even had at his disposal a network of temperature soundings in Europe. On 1 Oct. at 14 M.E.T., a Low (A in Fig. 16a), produced in the ordinary way by the occluding of a frontal wave, with 978 mb at the centre, was lying over Denmark. The Low was already thermally symmetric in its interior, and it had begun to fill (' A degeneriert '). The vast and oval rain-area, much too wide for an ordinary frontal rain and nearly concentric with the low-pressure region, might be taken as a sign of the initiation of an unusual development. Indeed, these two facts indicated that an orderly lifting and overturning of the air within the Low had already started. Next morning the Low had regenerated and intensified, Fig. 16b, and the central pressure had fallen from about 980 to 963 mb, which is a very low pressure over the Baltic, especially in early autumn. Over the northern Baltic and the Bothnian Sea there was a gale or even a hurricane with continuous rain. Schröder explained this development by assuming that extra energy was added to the system, not from the latent heat of condensation, but from an Arctic front that had come in from the north-west. It seems more natural, however, to explain it by the regeneration effects discussed below since the regeneration occurred at night and over sea. Anyhow, this case is a typical example of the unexpected formation of a new intense vortex at the centre of an old one.

No addition of extra frontal energy from outside the system can be used as an explanation of the partly unexpected development over Europe from the evening of 7 July 1929 to the morning of 8 July. A Low over Poland was already occluded on the evening of the 7th and was hardly deepening any longer. Pressure was above 995 mb at the centre, and no other disturbance or new front was within reach. In the morning of the same day the Weather Service at Stockholm had forecast weak to moderate winds and mostly no rain during Monday in eastern Sweden. In the evening the forecast was changed to ' north-easterly fresh or strong wind and general rain.' Next morning the cyclone had regenerated over the Baltic, with a central pressure of 980 mb, and along the coast outside Stockholm there was a gale to full storm during the day.

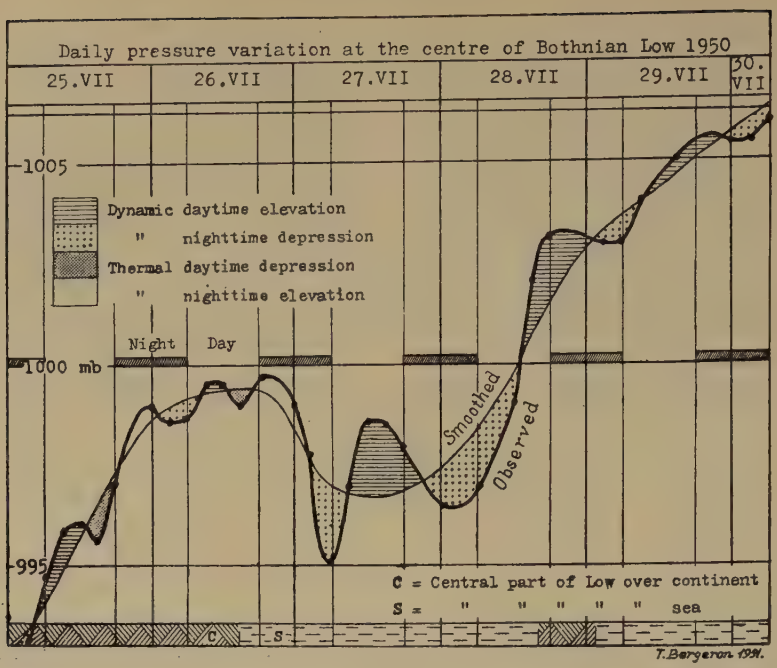


Figure 17. Pressure variations of Bothnian Low, July 1950, showing night-time cyclogenetic effect over sea.

Fig. 17 gives further proof of the existence of the effect postulated and throws light on its mode of occurrence. The central part of the Scandinavian Low of 25-30 July 1950 was lying over the Bothnian Sea for three days. This sea surface is evidently too small, however, to produce the full regenerating effect, especially if the lability energy of the air mass within the Low is not very great. Consequently, the expected dynamic night-time depression of the atmospheric pressure at the centre (clearly to be seen in Fig. 17 during three consecutive nights) could never sink below a certain critical threshold value (probably around 990 mb), and the cyclogenesis remained an abortive one. No doubt the pulsation between sea and land breeze contributes to the pressure oscillation shown in Fig. 17.

Two other intense summer cyclones, now being investigated as extra-tropical hurricanes, also occurred in July, namely in 1882 and 1931. As in the case of July 1929, the deepening took place over the Baltic and, because of the season, it cannot be explained by an addition of extra energy from an Arctic front. The regenerations of the type described above happen mostly during night over sea because the air then becomes more unstable in such regions, presumably in analogy with the formation of the tropical hurricanes.

In all five cases exceptionally great rainfall occurred in the form of continuous rain over a vast area. As shown by Fig. 18 the storm of July 1929 in the 24 hours from 08 M.E.T. on the 7th to 08 M.E.T. on the 8th gave as much as 30 to 65 mm of rain over southern Sweden. On 10 July 1882 more than 100 mm fell in 24 hours in some parts of Södertörn, cf. Fig. 19. This was, in fact, the greatest rainfall of this type during one day measured anywhere in Sweden during the period 1873-1910 (cf. Hamberg 1911).

The distribution and total amount of rain in the above cases indicate that the rain was produced by some kind of cyclonic rain mechanism capable of a much greater yield than the ordinary frontal one. The latter kind of mechanism will in our climate, even in summer, hardly produce a greater average rain intensity than 1 mm/hr unless

Figure 18. Rainfall from Baltic summer hurricane 7 to 8 July 1929.

it is combined with some local orographic or convective mechanism. Especially in the case of Fig. 18 – but also in Fig. 19 – the 24-hourly yield exceeded 60 mm, or 2·5 mm/hr, within a vast maritime region (the northern Baltic between Gotland and the Swedish mainland), and it was less inland, proving that in this region no such local mechanism can have contributed to the great yield. Most likely then, a mechanism akin to a tropical hurricane was responsible for the excessive rain; cf. § 8.

The five cyclones just discussed were, of course, not real hurricanes. (With the present climatic conditions true hurricanes will never occur at such high latitudes). No ' eye ' was observed within them, the hourly yield of rain at sea was at the utmost 4 mm/hr (instead of 25 mm/hr in a true tropical hurricane), their wind velocities were inferior and their diameter much superior to that of the real hurricanes.

The absolute humidity at the core of the October storm, 1921, over Denmark, was as high as 10 g/m^3. Consequently, a mechanism kinematically identical to a true hurricane, but fed by this air, should be able to produce a rainfall of about 12 mm/hr. Thus, within the main updraught zone, w is at least three times smaller, but the zone itself much vaster in the extra-tropical hurricanes than in the tropical ones.

Is this difference to be explained purely thermodynamically ? In September-October, quite deep and cold Polar air masses may reach the waters between 50° and 60° N. where the sea surface may still be almost as warm as in summer, thus conditioning a high degree of static instability. Moreover, the higher value of the moist adiabatic cooling γ' at lower temperatures is compensated by a correspondingly higher value of the vertical lapse rate γ_{PL} in the polar air.

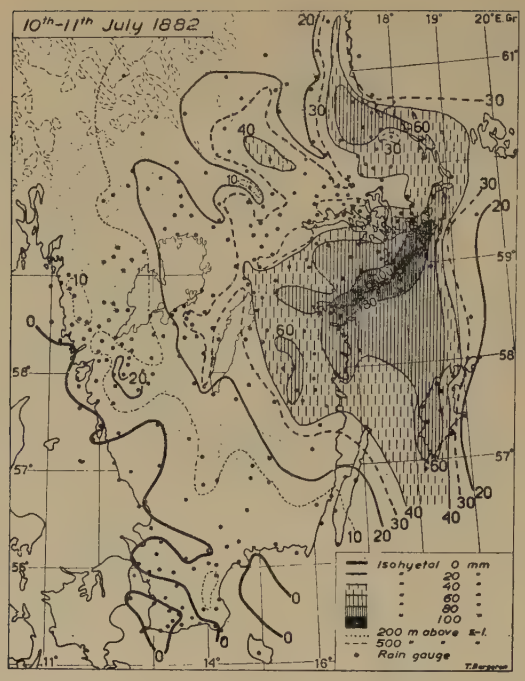


Figure 19. Rainfall from Baltic summer hurricane 10 to 11 July 1882.

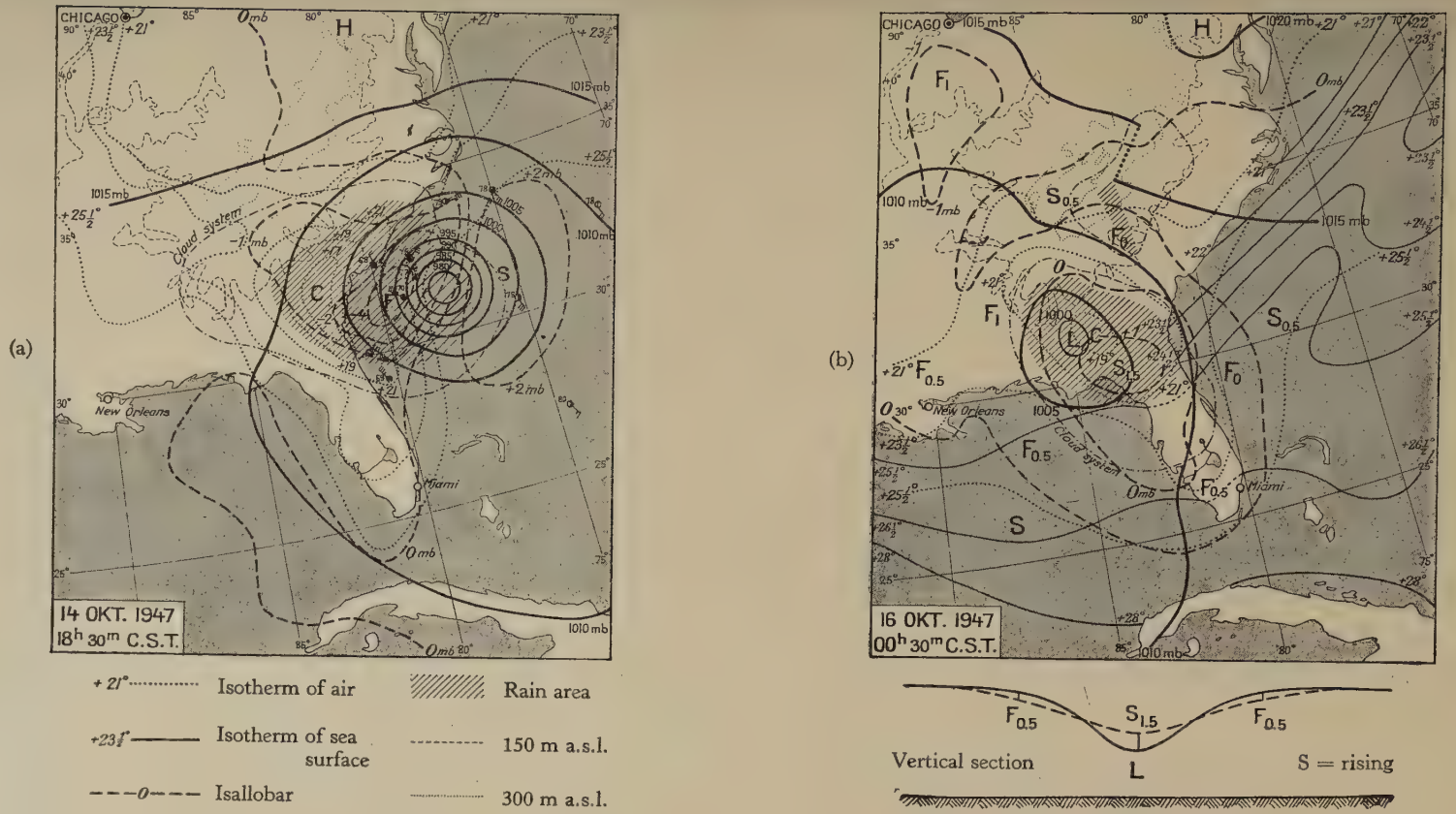
To the present writer, the difference between these two classes of hurricanes, which after all is a qualitative one, seems mainly conditioned by the difference in Coriolis force f. At low latitudes the low value of f permits the air at the periphery of a cyclone to reach the centre within a few revolutions, without losing too much of its angular momentum by friction. Thus, the warm-air funnel and the ' eye ' of the cyclone are formed, and the cloud system gets its characteristic shape. The main rain zone is removed to a position outside the zone of updraught whereby the two cycles O_E and O_I may co-operate and a mechanism of the utmost efficiency and dynamic stability is formed.

Within the extra-tropical hurricanes, of the type described above, the stage just described is never fully attained, and in the cyclones forming over sub-tropical seas and coming inland at such latitudes this stage is replaced by the structure shown in Fig. 3. In all these cases, as well as with some other types of rain mechanisms, cycles corresponding to the types O_E and O_I will partly counteract each other. The result is invariably a lesser specific power, precipitation and dynamic stability of the mechanism.

11. The decay of a hurricane

Case of 12 *to* 17 *Oct.* 1947

As mentioned above, nobody has yet, on really good synoptic maps, followed the formation of a hurricane in detail since this process always occurs over sea. A related process occurs, though, when a tropical hurricane comes inland and thereupon rapidly degenerates. It is evident that the process of degeneration cannot be a repetition of that of the formation in the opposite order. But it ought to be possible to learn something concerning the formation of hurricanes from a study of their degeneration. The hurricane of the 12 to 17 Oct. 1947 (Figs. 20a and b) which reached the United States as a major hurricane, was quite interesting in this respect.



Figures 20 (a) and (b). The tropical hurricane over the south-eastern states of U.S.A., 14 Oct. at 18 C.S.T. and 16 Oct. 1947, at 00 C.S.T.

The maps show the rapid filling of the hurricane Low after its passage, on the 15th, from the ocean inland. In Fig. 20 (b) the filling is also demonstrated by the change of profile of an isobaric surface in a vertical section. The full line represents its shape on 15 Oct. at 2230 C.S.T., the dashed line on 16 Oct. at 0030 C.S.T.

On 14 Oct., at 18 c.s.t. (Fig. 20a), the hurricane vortex was still vigorous, with a central pressure of about 976 mb just off the Florida coast. Thanks to the warm sea surface with its continuous supply of heat, the air temperature at sea-level in the interior of the hurricane remained high; near 26°C even within the rain area. The areas of falling and rising pressure did not indicate any filling of the hurricane Low, but only a continued motion towards the west. Within the part of the rain area that had reached land, however, we can already distinguish a strong cooling of the lowest layers of the air (down to 19°C or lower), in spite of the general afternoon heating. Only 30 hours later (Fig. 20b), the centre is lying over Georgia. It has filled by about 25 mb and shows a pressure rise in its interior, and a compensating, very weak, ring-shaped region of falling pressure, concentric with the Low, having a radius of between 300 and 500 km.

Up till now the rapid decay of tropical hurricanes over land has always been explained by the increased friction. The effect of friction on the dynamics of the cyclone may be ambiguous, however, whereas the process illustrated by Fig. 20 can only have one interpretation – the hurricane has acquired a cold core, and its motion is now automatically braked, as hinted already in § 6.

We have also shown that in February over the Caribbean Sea an air temperature of 25°C at the sea surface is insufficient for driving the hurricane mechanism. In October over the south-eastern U.S.A., air with $\theta_w < 21$°C can no longer drive the hurricane mechanism; in other words, the stratification of the atmosphere in this region is then a stable one with respect to air of θ_w-values less than 21°C.

In fact, in the case of Fig. 20a, an assumption of a relative humidity over the sea not lower than 85 per cent within the hurricane Low, gives θ_w around 25°C near the hurricane centre, and $\theta_w \geqslant 21$°C everywhere inside the 995 isobar, at a distance of 200 km from the centre. At this stage, then, the hurricane was still driven convectively, a statement which coincides well with the observation that the centre was not yet filling. In the case of Fig. 20b, 30 hours later, the conditions were reversed. Even assuming saturation at all levels within the rain area, θ_w now remains < 21°C inside most of this area and within all the former zone of updraught. Therefore, the air approaching this zone can no longer keep the hurricane going according to the convective scheme; on the contrary, it must brake or stop the hurricane motion*, as was the case in reality.

Evidently, the rapid rise in the interior of a degenerating hurricane must be compensated by a pressure-fall elsewhere in the atmosphere. It is interesting, though, to be able to state that in this case the area of pressure compensation is concentric with the disturbance and located at less than 500 km from its centre. Moreover, one is here able to observe directly how a rather rapid change of a wind field, and its pressure field, within a restricted but not too small region, is spreading through the atmosphere in other ways than by advection. It is possible that the spreading in this case has the nature of a wave-like propagation of a field of motion, and of pressure, from the centre of perturbation – the rapidly decaying hurricane. Up to now such a propagation has been clearly and directly observed only with acoustic waves within the atmosphere. The dispersion of energy over long distances, studied by the Chicago School of recent years, may be of the same nature as the phenomenon noticed here.

12. Further outlook

Will it ever be possible to prevent the formation of tropical hurricanes? In any case, I do not think that such a project could be realized by causing artificial rain through

* In fact, Riehl expounds a similar principle in his paper of 1950.

any sort of seeding within the hurricane cloud. As we have seen, some kind of natural precipitation release is already always active within this cloud, and so artificial cloud seeding cannot change the state essentially. If it were possible, on the other hand, to cover the sea surface with some substance which might prevent the cool hurricane rain from sinking down through the warm sea-water, then the energy supply of the hurricane might perhaps be quenched, in the same way as is now the case over land, since the hurricane energy is derived from the immense supply of heat in the interior of the ocean. Luckily enough, however, it will probably not be possible for Man to find such a substance and thereby to change Weather in this respect.

Another problem, of much more far-reaching consequences, presents itself. What kind of secular changes may have existed in the frequency and intensity of the hurricane vortices on our Earth? And what changes may be expected in future? We know nothing about these things, but I hope that this paper may have shown that even quite a small change in the different factors controlling the life history of a hurricane may produce, or may have produced, great changes in the paths of hurricanes and in their frequency and intensity. A minor alteration in the surface temperature of the sun, in the general composition of the earth's atmosphere, or in the rotation of the earth, might be able to change considerably the energy balance and the balance of forces within such a delicate mechanism as the tropical hurricane. During certain geological epochs, hurricanes may have been just as frequent as the cyclones of our latitudes, or they may have occurred all over the oceans and within all coastal regions, and they may have been even more violent than nowadays. During other periods they may have been lacking altogether. In studying paleo-climatic and paleo-biological phenomena, especially along the coasts of previous geological epochs, it may be wise to consider such possibilities.

The hurricane problem is far from being solved, but perhaps it may now be expressed more concretely and in detail. We may now say where in time and space our observations ought to be concentrated, and what kind of observations we shall need for solving the problem – a much closer network of fixed stations than hitherto, giving temperature, humidity and clouds at all levels within at least one of the regions of hurricane origin. Moreover, for investigating the cloud system of the hurricane we need studies of its thunderstorm electricity, and particle size and distribution, with the aid of cathode-ray oscillographs, radar equipment and other electric instruments.

The achievement of such a programme is at present even more necessary than theoretical work in this field. The task is extremely important not only for practical weather service, but also for the whole science of geophysics, because the tropical hurricane is probably the most regular, the simplest, and most pronounced disturbance of great extent and intensity within any geophysical medium. Moreover, at the formation of a hurricane, and during its maintenance and propagation, practically all the essential meteorological factors and elementary processes known, and some oceanographic ones, are co-operating: the general atmospheric circulation, the radiation balance, the cycle of water vapour (including the precipitation release), the orographic conditions, the temperature conditions of the ocean, etc.

The tropical hurricane is one of Nature's magnificent geophysical experiments, which, in an exceptionally clear way and with a spectacular shape and mode of action, is continually repeated before our eyes – and nowadays also within reach of our instruments. The hurricane may eventually teach us how the atmosphere reacts to simple and intense impulses. We must utilize this opportunity to understand the relatively simple and clear phenomena in order later to be able to understand the more complicated, weaker and more diffuse ones, which ordinarily control weather and climate both at lower and higher latitudes.

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The computation of forecast charts by application of the Sawyer-Bushby two-parameter model

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SUMMARY

Two series of 12-hr and 24-hr forecast 500 mb, 1,000 mb and 1,000-500 mb thickness charts have been computed electronically using the Sawyer-Bushby (1953) two-parameter baroclinic model of the atmosphere. The results are compared with the actual charts, and a good degree of correspondence is found between the actual and computed charts.

1. INTRODUCTION

Several authors have recently suggested that the large-scale atmospheric motions could be predicted by the application of numerical methods of mathematical analysis to equations which represents the motion in various simplified models of the atmosphere. Charney and his colleagues at The Institute for Advanced Study, Princeton, have investigated the barotropic model atmosphere and Charney, Fjörtoft and von Neumann (1950) have given the results of some 12-hr and 24-hr forecasts of the 500-mb contour surface based on this model. More recently Charney and Phillips (1953) give the results of some 12-hr and 24-hr forecasts based on the two-layer barotropic model. Alternatively, some of the baroclinic features of the atmosphere can be incorporated in a simple two-parametric baroclinic model by assuming that the variation of the horizontal and vertical components of velocity with height is the same along all verticals. Eady (1952), Eliassen (1952), Sawyer and Bushby (1953) and Thompson (1953) have derived sets of equations which describe the motions in particular versions of this model.

The model of the atmosphere used by Sawyer and Bushby consists of a baroclinic fluid in which the thermal wind is constant in direction in any vertical column but not necessarily parallel to the wind direction at any level, and the thermal wind speed is assumed proportional to the pressure difference through the layer concerned. The atmosphere is considered bounded by two pressure surfaces across which the movement of the air is assumed to be negligible, one pressure surface, $p = p_1$, corresponding to a level near the tropopause and one, $p = p_0$, corresponding to a level near the ground. The variation of the vertical motion with respect to pressure is assumed to be parabolic, and the geostrophic approximation is made after the horizontal divergence has been eliminated from the equations.

The present paper gives the result of 12-hr and 24-hr forecasts based on the Sawyer-Bushby model for two synoptic situations but the computations in each case were done well after the event. The computed charts, subsequently referred to as forecast charts, are compared with the actual charts and an attempt is made to estimate the effect of computational instability and arbitrarily chosen boundary conditions on the answers.

2. EQUATIONS

The state of the atmosphere in the Sawyer-Bushby model is specified by the two parameters h_m, the height of the central pressure level of the layer from $p = p_0$ to $p = p_1$ (at $p = p_m$) and h', the thickness of the layer from p_0 to p_m. The method is one in which

the instantaneous rates of change of h_m and h' are computed for time t using values of h_m and h' at that time and from these calculated rates of change the values of h_m and h' are obtained for a later time $t + \delta t$. The process is then repeated n times and forecast charts are produced for time $t + n\delta t$.

Eliminating Π_m (where $\Pi = dp/dt$, a measure of vertical motion, and the suffix m refers to the value at the level $p = p_m$) from the equations derived by Sawyer and Bushby, and neglecting the term representing the vertical advection of vorticity, the following equations can be derived for $\partial h_m/\partial t$ and $\partial h'/\partial t$:

$$\nabla^2 \partial h_m/\partial t + J(h_m, \beta^2 gl^{-1} \nabla^2 h_m + l) + \tfrac{1}{3} J(h', \beta^2 gl^{-1} \nabla^2 h') = 0 \quad . \quad (1)$$

$$\nabla^2 \partial h'/\partial t + \frac{4g(\nabla^2 h_m + l^2/\beta^2 g)\{\beta^2 gl^{-1} J(h_m, h') + \partial h'/\partial t\}}{RA\,\Gamma_p (p_0 - p_1)} + J(h_m, \beta^2 gl^{-1} \nabla^2 h') + J(h', \beta^2 gl^{-1} \nabla^2 h_m + l) = 0 \quad . \quad (2)$$

where

$$A \equiv \frac{p_0 + 3p_1}{2(p_0 - p_1)} - \frac{4p_0 p_1}{(p_0 - p_1)^2} \ln \frac{2p_0}{p_0 + p_1},$$

∇^2 and J are the two-dimensional Laplacian and Jacobian operators referred to rectangular Cartesian co-ordinates in the stereographic projection of the northern hemisphere on to a plane perpendicular to the earth's axis passing through the north pole; β is the magnification factor $\sec^2(\pi/4 - \theta/2)$, θ being the geographical latitude; g is the acceleration due to gravity; l is the Coriolis parameter; R is the universal gas constant and Γ_p a measure of the departure of the lapse rate from the adiabatic given by $\Gamma_p = \partial T/\partial p - \gamma/g\rho$, γ being the appropriate wet or dry adiabatic lapse rate, ρ the density and T the absolute temperature. Justification of the neglect of the term representing the vertical advection of vorticity is given by Bushby and Hinds (1954).

The following system of equations was used for the time-integration process :

$$H_{n+1} = h_{n-1} + 2(\partial H/\partial t)_n \, \delta t \quad . \quad . \quad . \quad . \quad (3)$$

$$h_{n+1} = h_n + \tfrac{1}{2}[(\partial H/\partial t)_n + (\partial H/\partial t)_{n+1}] \, \delta t \quad . \quad . \quad . \quad (4)$$

where H_n represents the first approximation and h_n the second approximation to either h_m or h' at time $t + n\delta t$, and $\partial H/\partial t$ is the value of $\partial h_m/\partial t$ or $\partial h'/\partial t$ obtained from Eqs. (1) or (2) when the first approximations to h_m and h' are used in those equations. For the first step in the integration, values of h_m and h' are known only at one instant of time and it is necessary to replace the centred difference Eq. (3) by the forward-difference formula

$$H_1 = h_0 + (\partial h/\partial t)_0 \, \delta t \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It was decided to use Eqs. (3) and (4), instead of Eq. (3) alone, for the time-integration procedure in an attempt to reduce the effect of computational instability.

The computations were carried out partly on LEO, an electronic computing machine owned by Messrs. J. Lyons and Co. Ltd., and partly on a Ferranti machine at Manchester University. The finite-difference approximations to Eqs. (1) and (2) were solved for $\partial h_m/\partial t$ and $\partial h'/\partial t$ using the technique described by Bushby and Hinds (1954). A rectangular grid of 18 × 14 points was used. As two rings of boundary points are lost in computing the finite-difference approximations to the vorticity-advection terms, Eqs. (1) and (2) were solved for a grid of 16 × 12 points including boundary points.

As these computations were necessarily experimental rather than routine in nature, the results were printed after each step in the time-integration process. This increased

the length of time spent in completing one cycle of the calculation, but with recent additions of high-speed input and output equipment to LEO, it is now possible to complete one cycle of the computation in about nine minutes on either machine. As it takes about five minutes to get the programme and initial data into the machine, it would take approximately $(5 + 9n)$ min to complete a forecast for time $t + n\delta t$, given initial data at time t.

In the present work the values of p_0 and p_1 were taken to be 1,000 mb and 200 mb respectively. This would give $p_m = 600$ mb, a level for which charts are not normally available. h_m was therefore identified with the height of the 500-mb surface and h' with the thickness of the 1,000-500 mb layer.

After consideration of previous computational results a constant value, 38°F per 500 mb, was taken for Γ_p.

3. Space and time increments

In order that the solution of the finite-difference approximations to Eqs. (1) and (2) should approximate closely to the continuous solution, the space and time increments δs and δt must be small in comparison with the space and time scales of the large-scale atmospheric systems. However, the smaller δs, the more grid points are necessary to cover a given area, and the computation time is increased. After some experimenting it was decided to use a value of δs which approximates to 260 km in middle latitudes.

Apart from the aforementioned considerations, it is also necessary to ensure that the small-scale irregularities in the fields of h_m and h' do not amplify during the integration process. The computation is said to be unstable when such amplification takes place. Owing to the complex nature of the equations involved, it is extremely difficult to derive rigorously the conditions necessary for stability, but it is apparent that the ratio $\delta s/\delta t$ has to lie within certain limits.

From general considerations, it was decided to commence the computations with δt equal to two hours. It could be seen from inspection of the results that the integration based on the initial data of 15 GMT 27 January 1952 was unstable after the 18-hr stage. The distortions had increased in amplitude by the 24-hr stage but did not affect the region of the British Isles, for which a reasonable 24-hr forecast was computed. The computations were then repeated but δt was changed to one hour when the 12-hr stage was reached. This removed all trace of instability (see Figs. 1-3).

During the integration based on the initial data of 15 GMT 14 March 1949, instability became apparent after the 10-hr stage and considerable amplification and distortion over most of the chart had taken place by the 12-hr stage. The computation was stopped and restarted from the 8-hr stage, using δt equal to one hour. The calculations then remained stable over the whole of the chart for the remainder of the forecast period (Figs. 4-6).

Charney *et al.* (1950), when solving the barotropic equation, using a centred-difference formula for the time integration, derived the following approximate relationship

$$\frac{\delta s}{\delta t} \geqslant \sqrt{2}\,\beta\, v \qquad (6)$$

where v is the maximum particle speed in the forecast region, as giving a lower bound to the stability criterion. If the same relationship is applied to the present set of equations, then with δt = two hours, the computations would become unstable if v were greater than 60 kt in middle latitudes. It is thought that the present method of time integration is more stable than the simple centred-difference formula used by Charney *et al.* (1950), but the problem of computational instability is at present being further investigated in the Forecast Research Division at Dunstable.

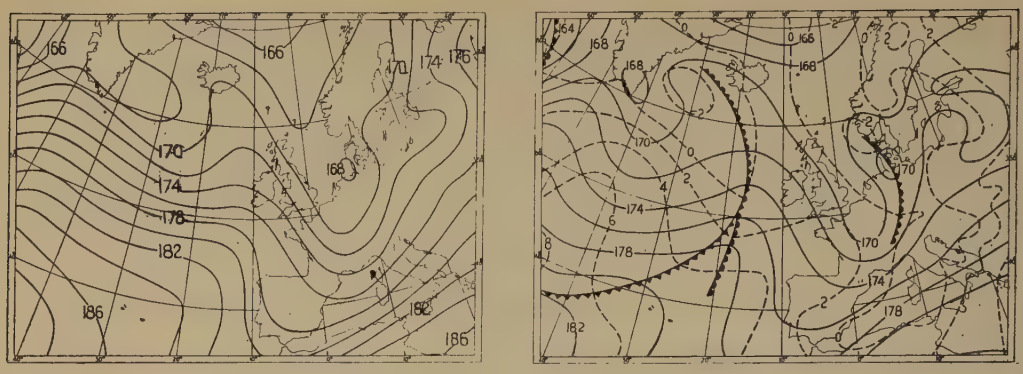

Figure 1. 15 GMT, 27 Jan. 1952. (a) Actual 500 mb chart; (b) Actual 1,000 mb and 1,000-500 mb thickness charts. ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours.

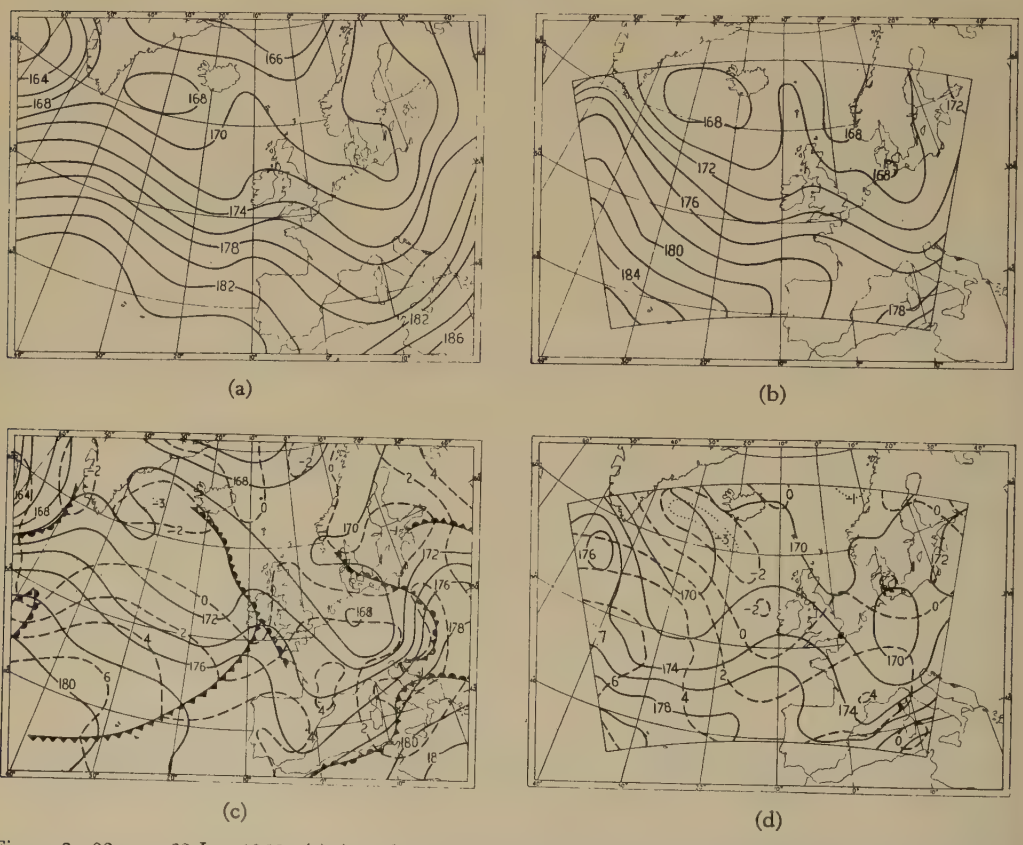

Figure 2. 03 GMT, 28 Jan. 1952. (a) Actual 500 mb chart; (b) Computed 500 mb chart; (c) Actual 1,000 mb and 1,000-500 mb thickness charts; (d) Computed 1,000 mb and 1,000-500 mb thickness charts. ((c) and (d) ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours)

4. Boundary conditions

A sufficient boundary condition for the solution of Eqs. (1)-(5) in a given region is that h_m, h', $\nabla^2 h_m$ and $\nabla^2 h'$ should be given along the boundary of that region. Thus when the finite-difference approximations to Eqs. (1)-(5) are being solved, it is necessary

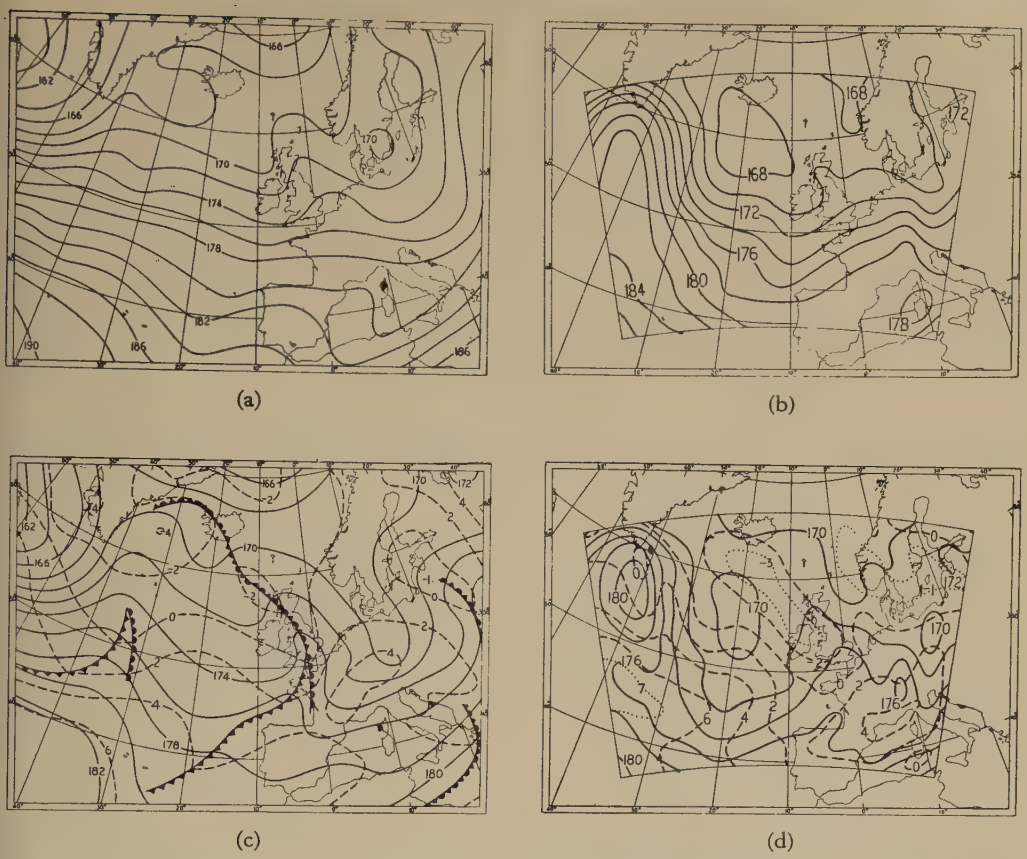

Figure 3. 15 GMT, 28 Jan. 1952. (a) Actual 500 mb chart; (b) Computed 500 mb chart; (c) Actual 1,000 mb and 1,000-500 mb thickness charts; (d) Computed 1,000 mb and 1,000-500 mb thickness charts. ((c) and (d) ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours)

to specify the values of h_m, h', $\nabla^2 h_m$ and $\nabla^2 h'$ at all grid points on the boundary (except corner points) or alternatively to specify the values of h_m and h' at all grid points on the boundary and at all grid points immediately exterior to the boundary. The second of these two alternatives was used in the present computations.

It was considered that the arbitrary boundary values would not affect significantly the centre of the forecast region by the end of the forecast period. The boundary values assigned to h_m and h' have to be chosen quite arbitrarily, because unless one considers the whole of the northern hemisphere in order to make use of the observed fact that h_m and h' show little variation near the equator, there are no conclusive reasons for pre-determining the values of h_m and h' along the boundary. The following boundary values were chosen as they are quite simple and tend to smooth the field of h_m and h' in a direction normal to the boundary in the vicinity of the boundary.

The initial data used in the computations were the values of h_m and h' at the points of the 18 × 14 grid. This enabled the Poisson and Helmholtz Equations (1) and (2) to be solved for $\partial h_m/\partial t$ and $\partial h'/\partial t$ at 16 × 12 points, including the boundary points at which values of $\partial h_m/\partial t$ and $\partial h'/\partial t$ were assumed zero. However in using Eqs. (3), (4) and (5) for the time integration, $\partial h_m/\partial t$ and $\partial h'/\partial t$ were taken as zero at the boundary points of the 18 × 14 grid; at the boundary points of the internal 16 × 12 grid, $\partial h_m/\partial t$ and $\partial h'/\partial t$ were now taken equal to half the value at the immediate interior point except that

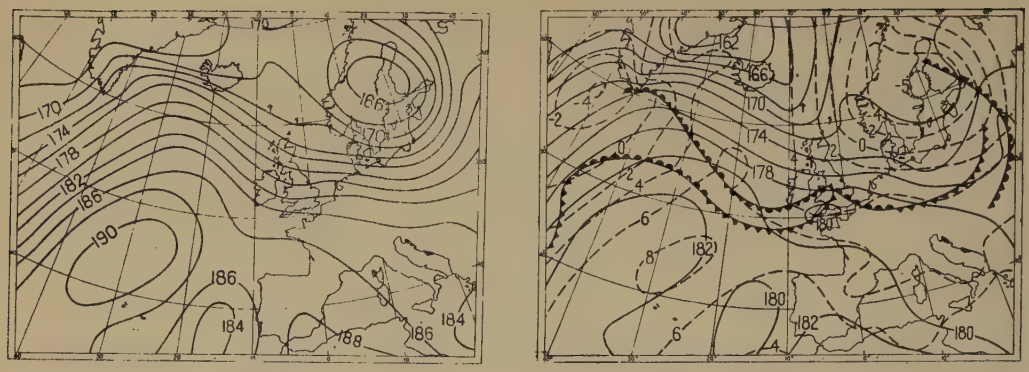

Figure 4. 15 GMT, 14 Mar. 1949. (a) Actual 500 mb chart; (b) Actual 1,000 mb and 1,000-500 mb thickness charts. ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours.

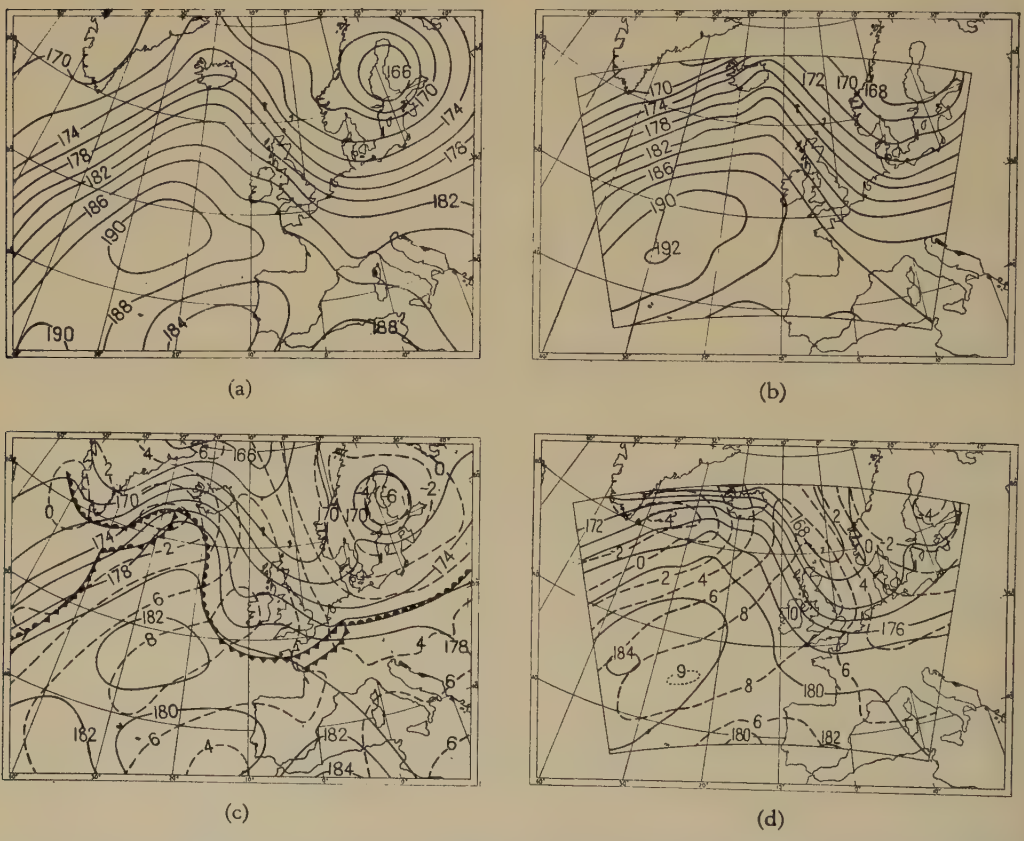

Figure 5. 03 GMT, 15 Mar. 1949. (a) Actual 500 mb chart; (b) Computed 500 mb chart; (c) Actual 1,000 mb and 1,000-500 mb thickness charts; (d) Computed 1,000 mb and 1,000-500 mb thickness charts. ((c) and (d) ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours)

at the four corner points a value of one-quarter of that at the nearest interior point was used. In the accompanying diagrams the computed results are shown for the area represented by the 16 × 12 internal points of the grid (this area does not appear rectangular as a conical orthomorphic background chart has been used).

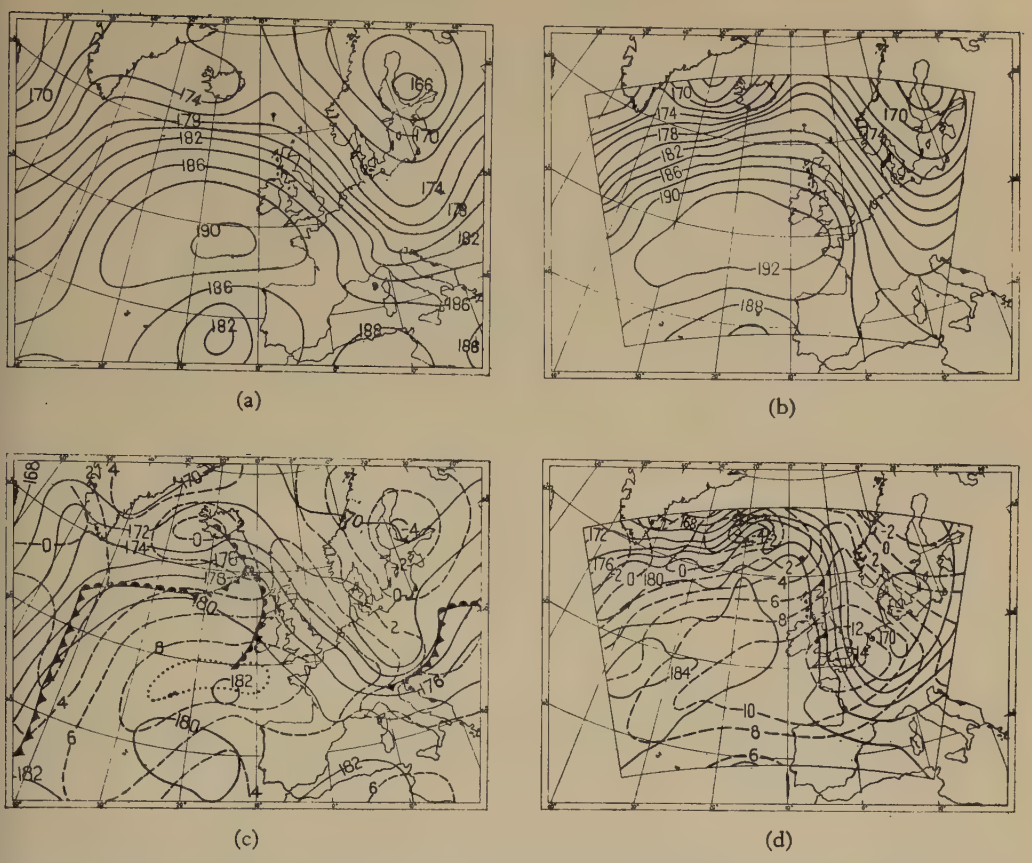

Figure 6. 15 GMT, 15 Mar. 1949. (a)Actual 500 mb chart; (b) Computed 500 mb chart; (c) Actual 1,000 mb and 1,000-500 mb thickness charts; (d) Computed 1,000 mb and 1,000-500 mb thickness charts. ((c) and (d) ———— 1,000-500 mb thickness lines, – – – – 1,000 mb contours)

5. Results

(a) *Time-integration based on initial data for* 15 GMT 27 *January* 1952

The initial charts on which this computation is based are shown in Figs. 1 (a) and 1 (b). The situation was one in which there was a broad westerly flow across the North Atlantic at 500 mb, with large-amplitude troughs over eastern Canada and western Europe and an intermediate shallower trough south-east of Greenland. Surface and thickness patterns were also largely westerly over the Atlantic, but over western Europe the surface-pressure gradient was flat and mainly cyclonic, there being a tongue of deep cold air down the North Sea into northern France.

During the ensuing twenty-four hours the cold trough over the North Sea moved slowly eastwards to Scandinavia and eastern Germany, the associated surface trough also moving away eastwards. The movements of this feature were well forecast, the axis of the 500 mb trough being in almost the correct position on each of the forecast charts (Figs. 2 and 3). The surface low-pressure area, however, was not forecast to move far enough eastwards. Behind the base of the cold trough, the ridge originally over the Bay of Biscay was forecast to intensify considerably and move eastwards to reach a position over northern Italy after twenty-four hours. The ridge moved as forecast for the first twelve hours but thereafter, possibly owing to the close proximity of the Alps, moved more north-eastwards than forecast. This is a particularly marked example of a

circumstance in which an experienced forecaster would almost certainly use his knowledge of the topography to modify a forecast produced by numerical methods. The elongated frontal trough to the west of Ireland intensified and moved eastward over the British Isles slightly faster than forecast, but the axis of the warm ridge was correctly forecast to be over Great Britain after twenty-four hours. The separate low-pressure centre, forecast to develop in the lower end of the trough, did in fact form at the point of occlusion shortly after the end of the forecast period.

During the forecast period the deep cold trough over eastern Canada moved eastwards across the boundary of the area involved in the computations. Since, however, the values of h_m and h' along this boundary were assumed to remain constant, the cold trough was assumed to remain over America and an intense warm ridge was forecast to be thrown up ahead of it. This gave rise to very tight gradients just south of Greenland on the forecast charts and, when δt was taken equal to two hours for the whole computation, small, intense irregularities appeared downstream on the 24-hr forecast charts due to computational instability.

(b) *Time-integration based on initial data* 15 GMT 14 *Mar.* 1949

Figs. 4 (a) and 4 (b) show the initial charts used in this forecast. A large stationary depression was situated over the Baltic in a very slack thermal gradient, whilst a vigorous depression was approaching the southern tip of Greenland from the south-west. Between the two a considerable ridge extended northwards from the Azores high-pressure area. There was a small wave-depression over southern England and a shallow low-pressure area with an almost concentric cold pool south-west of Spain.

In this situation the movements of various pressure systems were quite well forecast, but the predictions of their development and intensity were considerably in error (Figs. 5-6). It is particularly noticeable how similar the forecast and actual patterns are at 500 mb, the main errors in the forecast being the intensification of the ridge north of the Azores and the small trough west of Iceland.

During the forecast period the wave depression moved rapidly away into Russia and was followed by a mobile ridge which spread down into southern England and France and was absorbed into the considerably enlarged Azores anticyclone. On the forecast charts the oscillation in the thickness pattern was predicted to intensify too much, giving a very strong westerly thermal gradient over Iceland and northerly thermal gradient over the British Isles. This oscillation gave a very marked anticyclonic development area over the British Isles and in consequence the mobile ridge was forecast to develop into an extremely intense high-pressure area with the Azores anticyclone as a ridge extension.

The depression approaching the tip of Greenland at the beginning of the period was forecast to split into two parts, a shallow low-pressure area in the Davis Strait and a more intense depression moving north-eastward to Iceland. This movement actually occurred but the depression over Iceland was less deep that it was forecast to be.

(c) *Correlation and regression coefficients*

Correlation coefficients have been calculated comparing computed changes with actual changes over 12-hr and 24-hr periods starting from the initial data. Regression coefficients were calculated for the best fitting straight lines giving the actual change (Y feet) in terms of the computed change (X feet) in the form

$$Y = bX + c \quad . \quad . \quad . \quad . \quad (7)$$

In order to gain an indication of the adverse effects of the wrong boundary conditions on the western part of the forecast charts, correlation and regression coefficients were calculated both for the 16 × 12 grid of points over which the contour heights and thicknesses were forecast to change, and also over the same area without the six westernmost columns of points.

TABLE 1. CORRELATION AND REGRESSION COEFFICIENTS OF ACTUAL AND COMPUTED HEIGHT AND THICKNESS CHANGES

Forecast period commencing	Forecast period ending	grid size	500mb r	500mb b	500mb c	1,000 mb r	1,000 mb b	1,000 mb c	1,000-500 mb r	1,000-500 mb b	1,000-500 mb c
	03 GMT	16 × 12	0·41	0·43	36	0·35	0·47	− 3	0·55	0·46	24
15 GMT	28 Jan. 52	10 × 12	0·82	0·98	92	0·71	0·67	60	0·77	0·78	− 4
27 Jan. 52	15 GMT	16 × 12	0·18	0·15	29	0·49	0·67	− 15	0·42	0·27	30
	28 Jan. 52	10 × 12	0·84	0·72	139	0·79	0·78	101	0·76	0·56	28
	03 GMT	16 × 12	0·66	0·58	− 30	0·81	0·59	7	0·78	0·82	− 51
15 GMT	15 Mar. 49	10 × 12	0·84	0·83	− 47	0·90	0·70	− 22	0·85	0·80	− 8
14 Mar. 49	15 GMT	16 × 12	0·69	0·59	− 97	0·76	0·47	− 33	0·81	0·64	− 44
	15 Mar. 49	10 × 12	0·76	0·71	− 133	0·83	0·52	− 68	0·94	0·68	− 1

r = correlation coefficient

The figures in Table 1 confirm what has been said in Sections 5 (a) and 5 (b). The most striking features of Table 1 are the high correlations obtained if the six westernmost columns of the grid are ignored.

6. Conclusions

Although one must not generalize from two examples, the equations describing the motion in the Sawyer-Bushby two-parameter baroclinic model atmosphere give, in the two situations so far considered, a fair representation of the motion in the real atmosphere for a 24-hr period, in the regions not affected by the arbitrary choice of boundary conditions. In order to avoid computational instability it appears necessary to use a time step of one hour, but even so it is possible to compute a 24-hr forecast in under four hours. If the present rate of development of electronic computing machinery is maintained, it should be possible to make a significant reduction in this time.

Acknowledgments

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The terminal velocity of snowflakes

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Summary

Measurements of the terminal velocity of aggregate snowflakes at the ground indicate that the velocity is approximately proportional to the one-tenth power of the mass. The constant of proportionality is dependent on the basic crystal types in the aggregates. It is moreover greatly increased by riming or by slight melting of the snowflakes.

When snow grows markedly at a generating level, the rate of fall of the snow particles as determined from radar observations is found to correspond to the velocities of aggregates. It is thus inferred that aggregation can occur at temperatures far below the freezing point.

1. Introduction

Snowflakes, as observed at the earth's surface, are usually found to fall in the form of aggregates of many individual crystals. It has been thought that all types of snow crystals fall considerable distances before aggregating, the clusters being produced, by the collision and adhesion of the crystals during their fall, when the temperature approached 0°C. Radar observations suggest that, when the snow is generated in compact cells, aggregation may occur within these generating cells. Thus a relatively complex picture may exist, aggregation taking place at the generating level and again when the temperature approaches the melting point, with secondary processes being possibly effective throughout the fall of the snowflakes.

There is a need for velocity information to supplement radar studies. Schaefer (1947) and Nakaya and Terada (1935) have measured the velocity of single crystals, but the behaviour of aggregate flakes must be studied if they occur over a much wider range of heights than previously thought. A method was therefore developed whereby the mass and velocity of descent of snowflakes could be determined simultaneously. Interpretation of radar information requires a knowledge of the size distribution of snowflakes also, and a sample in space can be related to a sample collected at the ground only by knowing velocity as a function of size.

2. Methods of measurement

(*a*) *Velocity of fall*

The velocity of fall of a particular snowflake was measured directly from the time interval taken for it to fall through a given vertical distance. The moving snowflakes were photographed, against a dark background, from a distance of about 3 m, using a 16 mm cine-camera. Each snowflake produced a short streak (due to its motion while the lens shutter was open) on every frame of the film (Fig. 1). A camera speed of 32 frames per second, giving a measure of the time of fall, was found most suitable. At this speed, the streaks were not too long and the natural illumination available was sufficient for film of normal contrast to be used. The camera speed was carefully checked before and after the winter's operations by means of a stroboscopic flash unit and was found to be reliably constant.

Since the observations were taken outdoors, all the snow falling between the camera and the dark background in the volume defined by the angle of the camera lens was

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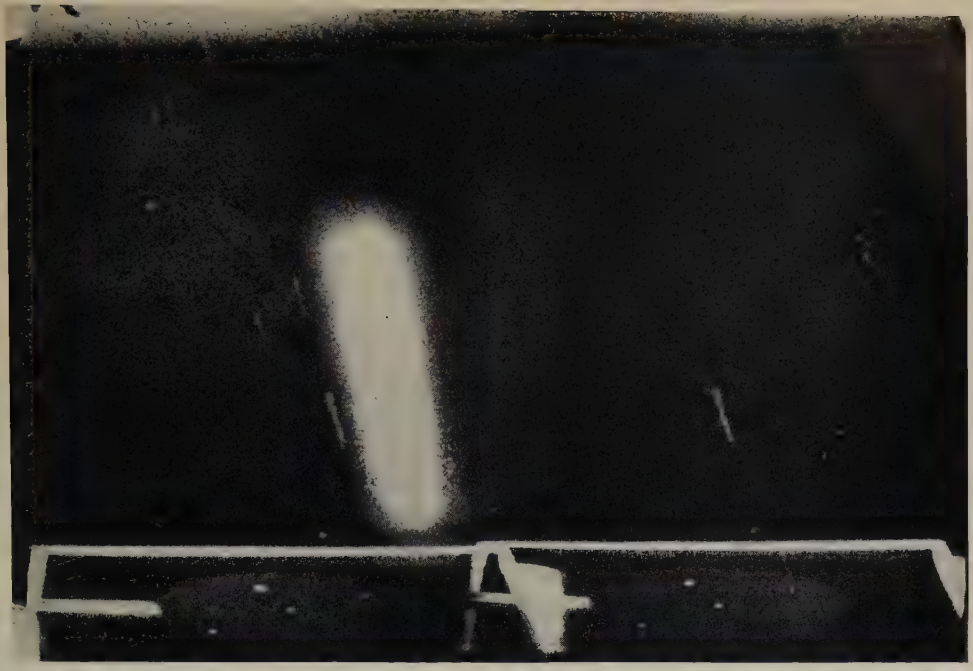

Figure 1. A single exposure of the 16 mm film using the stereoscopic attachment. The two images are shown side by side. The horizontal wool-covered disc with extending handle is near the bottom. Several snowflakes which have settled on it are seen as white spots. The falling snowflakes are seen as faint streaks against the vertical black background. The large blurred image on the left was caused by a snowflake falling very close to the left aperture of the stereo-attachment.

Figure 2. A sequence of 16 consecutive exposures showing a snowflake gradually settling on to the wool-covered disc. For ease of illustration, a very light snowfall has been chosen.

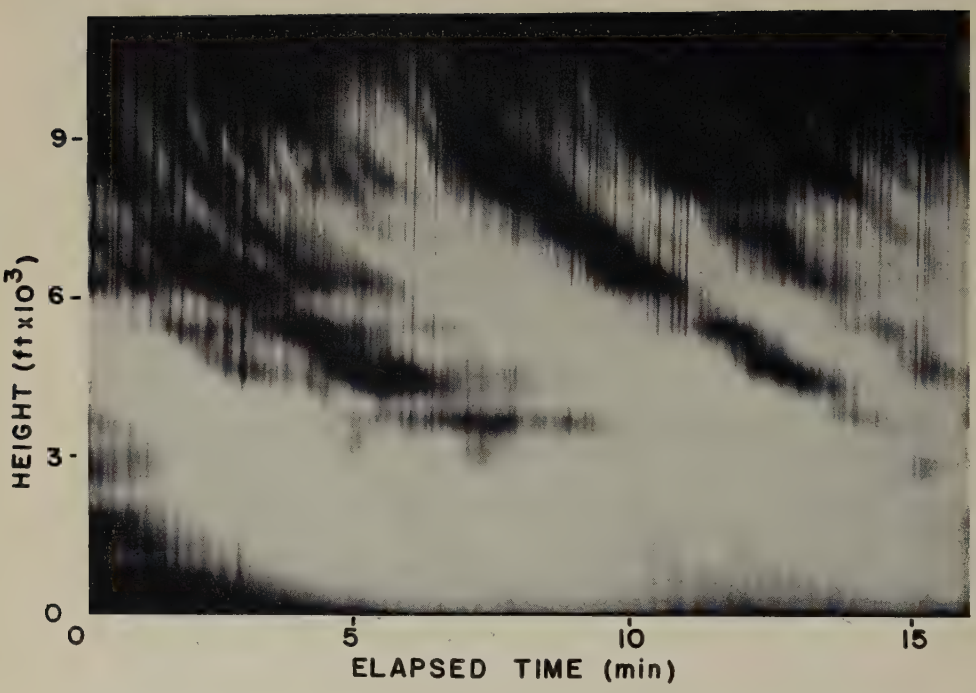

Figure 6. A typical snowstorm pattern recorded by the zenith-pointing radar (see p. 178).

photographed. Snowflakes close to the camera produced very large blurred images on the film and tended to obscure flakes which were more distant from the camera. A stereoscopic attachment,* originally intended for the study of any motion of the snowflakes along the axis of the camera, was mounted in front of the camera lens. This attachment consisted of two systems of reflecting mirrors and prisms which viewed the field from points separated by about 7 cm. Two images from slightly different angles were thus obtained side by side on the 16 mm film. A snowflake falling very close to one side of the stereo-attachment would still permit the other side to obtain an unobstructed view (Fig. 1).

Velocity measurements were made only for the snowflakes which were deposited on the horizontal wool-covered disc shown in Fig. 1. The films, after processing, were projected through a special type of projector† which could move the film, frame by frame, with perfect registration, as slowly as desired. The streaks due to a falling snowflake could then be followed on successive frames until it settled on the disc (sequence Fig. 2) and thus a velocity could be associated with a particular flake. When any wind was present, the camera was set up to photograph the descending flakes from crosswind, thus eliminating errors which could be caused by the change in magnification with distance from the camera. It was found unnecessary to use stereoscopy for this purpose. Minor corrections, depending on the position of a flake on the disc (i.e. on its distance from the camera), were also applied to the velocities.

(*b*) *Melted diameter and crystal types*

The collecting disc was 33 cm in diameter and was covered with black angora wool. The wool was brushed before each exposure so that the aggregates landed gently and were not shattered or moved from their points of arrival. The disc was sheltered from further exposure to the snowfall at the instant that the camera was stopped, and was then taken indoors. A Whatman No. 1 filter paper, previously dusted with powdered dye, was placed on the disc and absorbed the water from the melted snow particles. The calibration curve of Marshall, Langille and Palmer‡ (1947) was then used to relate the size of the stain on the filter paper to the diameter of the water-drop equivalent in mass to the snowflake.

This diameter, which will be called ' melted diameter ' is of significance for these reasons. The mass is proportional to the cube of this diameter, whereas the relation between mass and dimensions of an aggregate would be more complex, depending on crystal type, density and so on. Furthermore, it is common practice to use diameter in discussing size distributions and terminal velocity of raindrops, and, in radar theory, this parameter is one of the quantities affecting the reflectivity.

The pattern of stains on the filter paper was such that it was easily identified with the pattern of the snowflakes photographed on the wool disc. Thus each velocity measurement could be related to a snowflake of known melted diameter. Microscopic observations of the snowflakes were made at frequent intervals to determine whether they consisted of one or more basic crystal types. No attempt was made to identify the type of each individual flake before melting. A random sampling was enough to show that, for example, a snowfall might consist only of dendrites.

* Nord 3rd Dimension Converter, The Nord Co., Minneapolis, U.S.A.

† Dunning Animatic Projector, Dunningcolor Corp., Hollywood, Cal., U.S.A.

‡ This calibration curve was carefully checked at frequent intervals within the melted diameter range of 0·08 cm to 0·5 cm. The method found most convenient was to place a small water drop on a waxed glass slide. The drop assumed the shape of a perfect hemisphere when the glass slide was horizontal. Its diameter was measured with a travelling microscope and related to the diameter of the stain produced on the filter paper.

3. Observations and results

The following are some of the difficulties encountered in obtaining usable observations and in the interpretation of the results.

(a) On many occasions it was found that the air temperature at the ground during a snowfall would rise above the freezing point. Velocity measurements, on such occasions of slight melting, would not be representative of snow velocities at temperatures below freezing.

(b) On some of the days when temperatures were below the freezing point, severe winds approaching blizzard conditions were present. Sudden ground gusts in the vicinity could make the measurements unreliable.

(c) The photographic method depended on the light scattered by each snowflake. The larger aggregates, having greater scattering cross-sections, were more easily registered on the film. The lower limit of detection was for melted diameters of about 0·04 cm. Since the distribution of snowflakes is usually such that there are large numbers of small ones and relatively few large flakes, the number of velocity measurements within any diameter interval is not indicative of the total number of flakes in that interval.

(d) Indirectly there was also discrimination between crystal types. For instance, in a snowfall containing dendrites and plates, the dendrites would be more easily photographed near the limit of detection since they are less dense than the plates and therefore have larger scattering cross-sections.

During the winter 1952-3, successful measurements were made on 6 days. The results are given in Figs. 3, 4 and 5. The data are well fitted by an equation of the type.

$$v = kD^n$$

where v is the velocity (cm sec^{-1}), D the melted diameter (cm), k a constant, for a given type of snowfall, having the dimensions cm^{1-n} sec^{-1}, and n an index also dependent

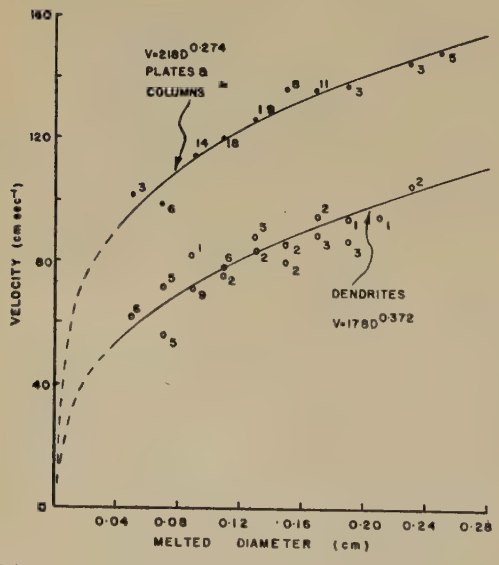

Figure 3. The lower curve is fitted to measurements obtained on 19 and 20 January 1953 when the surface temperatures were 28° and 30°F respectively and the snowfalls consisted solely of dendrites. The fall velocity of aggregates of columns and plates is shown in the upper curve, the observations having been made on 12 February 1953 at a temperature of 25°F.

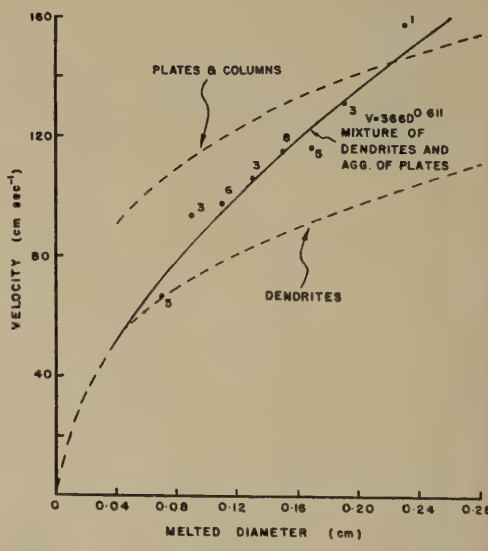

Figure 4. A mixed snowfall of dendrites and of irregular assemblages of plates occurred on 29 December 1952. The surface temperature was 26°F. The velocities show a tendency to approach those of dendrites for small sizes and of plates and columns for the larger sizes. The pecked curves have been transposed from Fig. 3.

on the type of snow. Each point in the diagrams represents the mean velocity within a diameter interval of 0·02 cm and has been placed at the centre of the interval. The number of measurements is indicated beside the plotted points.

The velocity-diameter curves of Fig. 3 may be considered as typical of snowflakes. It is seen that the fall velocity of aggregates of columns and plates is about 1·5 times that for dendrites of equal mass. The dendrites, which are feathery in appearance, have larger cross-sections and the air resistance to their fall is consequently greater than for the more dense plates or columns.

The solid curve of Fig. 4 is the result of measurements made on a mixed snowfall, consisting of a mixture of dendrites and of irregular assemblages of plates. The shape of this curve and its steep rise may be due to one or both of the following factors.

(a) The dependence of the photographic method on the scattering cross-section would heavily favour the registration of small dendritic clusters as opposed to small aggregates of plates, of equal mass. The velocities for particles of small melted diameter should then correspond to those found for dendrites as is seen to be the case.

(b) If the main aggregation was occurring among the plates, the dendrites would still predominate for the smaller sizes. For large sizes however the velocities should approach those of plates as in fact they do.

The effects of riming and very slight melting on the velocity of dendrites are shown in the curves of Fig. 5. Preliminary measurements at the end of the winter 1951-2 indicated that velocities of the order of 300 to 400 cm sec^{-1} are not uncommon at about 35°F, the precipitation still having the characteristic appearance of snow but being quite wet. The increase in terminal velocity for any snow particle is greatly dependent on the degree of riming and melting.

Recently Magono (1953) has presented results for the velocity of snowflakes. His observations on aggregates were, however, all obtained on one day over a period of eight hours during which the ground temperature fluctuated between + 2°C and − 8°C and the snowflakes were sometimes rimed. Only one basic type of growth, i.e. dendritic,

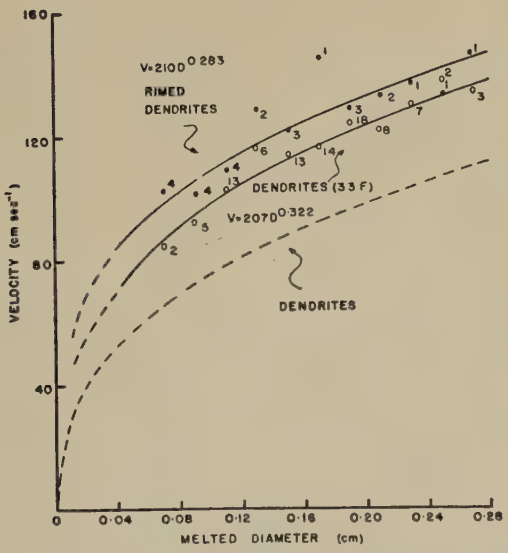

Figure 5. These curves show the effect of riming and of slight melting on the rate of fall of dendrites. The rimed dendrites were observed on 31 January 1953 when the surface temperature was 25°F, well below the melting point. Temperatures were slightly above the melting point on 6 December 1952. The displacement from the dry dendrite curve shown would in general depend on the amount of riming or melting.

was observed. The measurements were made visually and using a stop-watch, and the dimensions of the aggregates rather than their mass were used. Since the relation between dimensions and mass is unknown, it has not been possible to make a direct comparison of his results with those presented here. However, his velocities for unrimed dendritic clusters all exceed 100 cm sec^{-1}, a value attained here by only the largest clusters of dry dendrites. Indeed his data more closely resemble those shown in Fig. 5 for dendrites at a temperature slightly above the freezing point. For small sizes, his velocities are considerably greater than those obtained by other observers (e.g., Nakaya and Terada 1935).

4. Radar studies of snow patterns

Marshall (1953) has considered the precipitation patterns formed by snow when it is being generated continuously from a point source moving with the air speed at the level of generation. His theory has yielded a method for determining the rate of fall of the snow particles aloft from radar observations of the pattern made by the snow in falling through a wind shear. The velocity of fall of the particles at any height can be related to the slope of the pattern at that height and to the wind profile.

A fixed zenith-pointing radar* was used to observe the snow trails. The received signal was made to modulate the intensity of a stationary trace on an oscilloscope and the trace was recorded on film moving at constant speed at right angles to it. A continuous recording of the height of the echo (i.e. the intersection of the vertical radar beam and the snow pattern) against the time of observation was thus obtained. A photograph of a typical snow storm, obtained in this manner, is shown in Fig. 6 (Plate III).

TABLE 1. Showing the velocity of fall of the snow particles, at various heights, as determined from the radar echo and upper air winds.

Height ft	Wind kt		$v(z)$ ft sec^{-1}	Deviation from mean	Remarks
11,200	48		generating level		
9,200	41		2·94	0·05	13 Jan. 1953, 1515 GMT. Wind components in direction 280°. Generating level at 11,200 ft, − 10·5°C. Crystal types : irregular assemblage of columns and plates, capped columns, plates, very little aggregation.
7,200	38		2·85	− 0·4	
5,200	31·5		3·00	0·11	
3,200	23		2·77	− 0·12	
		mean	2·89	0·08	
12,000	60		generating level		28 Jan. 1953, 1515 GMT. Wind components in direction 220°. Generating level at 12,000 ft, − 14°C. Crystal types : needles, plates with simple extensions, some aggregation.
10,000	50		3·37	− 0·04	
8,000	39·4		3·96	0·55	
6,000	34·5		3·19	− 0·22	
4,000	28·5		3·13	− 0·28	
		mean	3·41	0·27	
15,500	48·25		generating level		2 Feb. 1953, 22 GMT. Wind components in direction 260°. Generating level at 15,500 ft, − 21°C. Crystal types : plane and spatial dendrites, plates with simple extensions, very large aggregates.
14,000	44		3·30	− 0·16	
12,000	38		3·56	0·10	
10,000	33		3·38	− 0·08	
8,000	18		(4·48)†		
6,000	15		3·58	0·12	
		mean	3·46	0·12	

(Conversion factors : 1 ft = 0·305 m, 1 kt = 0·515 m sec^{-1})

* Radar specifications : wave length 3·2 cm, peak power 40 kw, pulse length 0·75 μ sec, pulse repetition frequency 500 sec^{-1}, beam 2°.

† This high value is thought to be due to an error in the winds. The upper-air charts indicated the presence of a secondary front at 8,000 ft with a consequent sudden change in wind speed and direction through this frontal zone. An error of 500 ft in the estimation of the frontal height would bring the measured velocity of fall into line with the others.

Photographic radar records of three occasions have been analysed (Table 1). The pictures showed the generating elements and the ensuing snow trails. The rate of fall of the snow particles was determined from

$$v(z) = \frac{dz}{dt}\frac{w(z)}{W}$$

where $v(z)$ is the velocity of fall at depth z below the generating level,

$w(z)$ is the wind at depth z relative to the wind W at the generating level and along the same azimuth,

and $\frac{dz}{dt}$ is the slope of the pattern at depth z as measured directly from the film records.

The indication from Table 1 is that the velocity of fall of the snow particles, as determined from the radar echo and upper wind data, is reasonably constant with height on any given day. These velocities are moreover found to be of the order of 2 to 3 times greater than the velocity of individual snow crystals (average about 50 cm sec^{-1}) as measured by Schaefer and by Nakaya and Terada, being comparable with the velocities of aggregate snowflakes (Fig. 3). Careful ground observations of crystal types were made during the storms. No riming could be detected for the cases quoted so that there is little doubt that aggregate snowflakes did in fact exist at all heights indicated in Table 1 and therefore at extremely low temperatures.

The velocity of fall of the snow particles, as just discussed, is not to be confused with the rate of descent of the pattern overhead, as it passes through the beam of the radar. Taking the first case of Table 1 as an example, the rate of descent of the pattern overhead decreased from 615 cm sec^{-1} when the echo was at 9,200 ft to 160 cm sec^{-1} when the echo height was 3,200 ft.

(*a*) *Generating elements emitting polydisperse distributions*

The rate of fall of the particles is involved in the equation of the pattern and so a line pattern is obtained only for a single particle size, i.e. for a monodisperse size distribution. In the past, aggregation has usually been associated with wet snow and it was assumed that single crystals only would be found unless at some stage the snow fell through a region whose temperature approached the zero isotherm. Our radar studies indicate (Table 1) that when, as is commonly found, the snow is formed in compact cells, its velocity of fall, quite close to the generating level (i.e. before any significant change in size distribution has occurred), may be several times the velocity associated with single crystals and of the right order of magnitude for snow aggregates. This is evidence that on these occasions sufficient turbulence and updraught may exist in the region of formation to promote effective aggregation of the snow crystals, thus yielding a wide size distribution at temperatures far below the freezing point.

Starting with such a distribution at the generating level and assuming that no further aggregation or break up takes place during the course of fall, there would be a sorting of the snowflakes, according to size, with distance fallen. Each size, having its own velocity, would trace out a different trajectory, the resulting pattern in space broadening out as the distance of fall through the wind shear increased. A pattern of this kind, calculated for a high constant wind shear of 1 cm sec^{-1}/m, is shown in Fig. 7. Snowflakes of sizes ranging from 0·04 cm to 0·4 cm melted diameter have been considered. It is seen that the pattern broadens out rapidly, its width in the horizontal being about 68 km when the distance of fall is 4·5 km. At any instant, an observer at the ground could collect flakes of only one size, the size decreasing as the pattern passed him by. If, however,

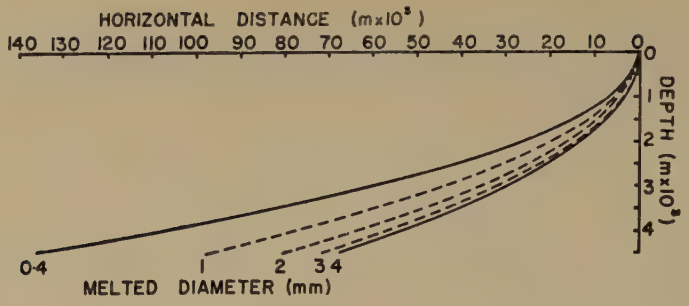

Figure 7. Showing the broadening of the pattern for snow particles of sizes 0·04 to 0·4 cm "melted diameter" generated continually at a point 4·5 km above the ground and falling through a constant wind shear of 1 cm sec^{-1}/m. Snowflake velocities were obtained from $v = 200\ D^{0.81}$.

there is a succession of such generating elements whose spacing is small compared to the horizontal extent of one pattern at the ground, the patterns could overlap a good deal. In this case, the observer collecting samples would expect a distribution of sizes, the distribution remaining practically constant throughout the storm except for a period at the beginning of the first pattern and for the duration of the tail of the last one.

On the other hand, it is probable that aggregates do break up and that new ones form by collision throughout the fall. The break-up of large aggregates would lead to fragments which would assume lower terminal velocities, and the newly formed aggregates would correspondingly increase their velocity. It is possible that some mean terminal velocity could be defined in this case and that the pattern due to a single generating element would broaden much less than for the example given in Fig. 7. Possibly a combination of the above mentioned effects may be acting simultaneously.

(*b*) *Significance of the terminal velocity deduced from the radar echo*

For a given range, the intensity of the signal from an assemblage of snow particles, which scatter incoherently, is very nearly proportional to the sum of the sixth powers of the melted diameters of the individual particles. Because of the dependence on the sixth power of the diameter, the small scatterers in a wide distribution, though very numerous, make an insignificant contribution to the radar echo as compared to the larger scatterers which are few in number.

It has been shown (Table 1) that the velocity of fall of the snow particles, as determined from the maximum intensity of the radar echo and the wind profile, is invariant with height on occasions when the snow is formed in long-persisting compact cells. A rapid interchange of velocities due to fragmentation and regrouping of aggregates could prevent broadening of the pattern. It is more likely that some interchange does exist but that the sorting of flakes by size predominates and that the radar echo arises from the contributions of the larger scatterers in a snow pattern. The observed radar echo would then represent a narrow band within the actual pattern and near its leading edge. If so, it is the mean velocity of these larger snowflakes that is presented in Table 1.

One must also consider the possibility that snowflakes can grow or evaporate slightly by sublimation during their fall. The velocity results of Table 1, though appearing reasonably constant with height, show enough deviation from their mean value to mask any such effect. If the mean deviation is taken to indicate the trend towards increasing velocity with distance fallen (certainly an upper limit), growths of the order of 15 per cent in melted diameter could have occurred.

6. Conclusions

The terminal velocity (cm sec^{-1}) of snowflakes from 0·04 cm to about 0·35 cm " melted diameter " (0·033 to 22·5 mg) is given by $v = kD^n$ (i.e., proportional to mass raised to the power of $n/3$). The index n varies slightly with crystal type, but may, as an approximation, be assumed equal to 0·31. The coefficient k is then 160 for dendrites and 234 for combinations of columns and plates, both values increasing rapidly with riming or wetting. For snowflake sizes below this range the technique used was unsatisfactory but the velocity measurements of other observers on single crystals appear to fit this formula within the accuracy of their experiments.

Radar analysis has revealed that for snow generated in compact cells, there may be little if any continual increase in snowflake size by aggregation during the course of fall. The velocities measured near the generating level are too great for single crystals so that aggregation must be occurring there at temperatures well below the freezing point. Since the generating zone seems to promote aggregation, the suggestion is therefore that it is a region of significant turbulence and updraught. This has been noticed in studies of time-lapse sequences of radar pictures which show that the generating elements are in a continuous state of internal motion.

Little information is available concerning the size distributions of snowflakes in space and the role of the various mechanisms that can affect them. Until these are better understood it is suggested that the size distributions in space may be best obtained from samples collected by gravitational settling at the earth's surface, by correcting the latter by the appropriate velocity-diameter curve.

Acknowledgments

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Airborne measurements of the concentration of natural and artificial freezing nuclei

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Summary

An aircraft-mounted instrument for measuring the concentration of freezing nuclei in the free atmosphere is described. It has been calibrated and its limitations investigated.

Preliminary observations, in flight, of natural freezing nuclei were made. The temperature at which a given concentration of ice crystals formed on natural nuclei varied markedly from day to day, and increased with altitude.

Measurements in flight of the concentration of freezing nuclei in silver-iodide smoke, released from a ground generator, are described. The nucleating properties of small particles produced by a hydrogen burner deteriorated very rapidly, while larger particles produced by a kerosene burner were more durable.

1. Introduction

Freezing nuclei are thought to play a part, by the process postulated by Bergeron (1933), in the formation of rain whether it forms by natural means or is artificially stimulated by seeding agents such as dry ice or silver iodide. Information is therefore needed concerning the distribution and properties of freezing nuclei in the free atmosphere. In recent years many observations of freezing-nucleus concentration have been made; a summary has been given by aufm Kampe and Weickmann (1951). Almost all of these measurements were made in cold boxes and expansion chambers at ground level. However, the real need is for observations in the upper levels of the atmosphere, where supercooled clouds occur in which freezing nuclei might initiate precipitation. These observations must necessarily be made in aircraft. To the authors' knowledge, the only freezing-nucleus determinations in aircraft which have been reported are those of Palmer (1949), who made a few measurements using small expansion chambers.

An instrument has been constructed, and mounted in an aircraft, to measure the concentration of freezing nuclei in the air through which the aircraft flies. It is described in this paper, together with laboratory tests of its performance and preliminary measurements made in flight.

The term ' freezing nucleus ' must be defined. In this paper it is assumed that if an ice crystal forms in a cloud of supercooled water drops at a certain temperature, then a freezing nucleus is present which is effective at that temperature.

2. Description of apparatus

The apparatus consists of a cold chamber, similar in principle to those described by other investigators (e.g., Schaefer 1948), in which freezing nuclei are detected when ice crystals form on them in a supercooled fog. There are three main component parts : an observation chamber, a warm reservoir and a cold reservoir.

The observation chamber is illustrated in Fig. 1. It consists of a cylindrical copper tank A, 46 cm in diameter and 46 cm deep; its volume is 76 litres. Its temperature is controlled by circulating alcohol through a coil of copper pipe B, 25 m long and 1·6 cm internal diameter, which is soldered to the outside of the chamber, forming a helix round

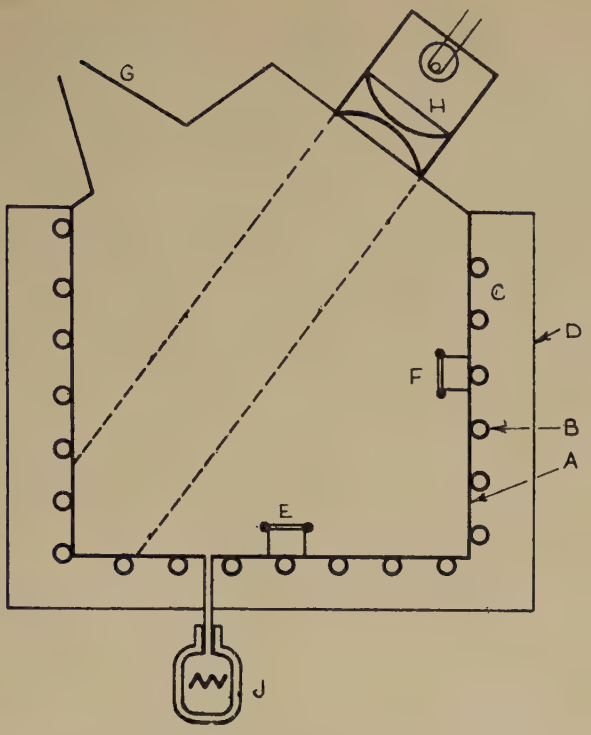

Figure 1. Observation chamber.

the walls and a spiral round the bottom. The chamber is lagged with 5 cm of expanded-rubber insulation C, in an outer tank D. The air temperature is measured by two thermistors E and F, mounted respectively 5 cm and 30 cm from the bottom of the chamber. The top of the chamber is covered by an insulated lid in which is mounted a rubber visor G, and a 250 w lamp with condenser lenses, H, giving a parallel beam of light. The light and visor are positioned to take advantage of specular reflections from crystals floating with an upper face horizontal. Fog is generated with an electric heater in a vacuum flask of distilled water J, communicating with the observation chamber by a Monel metal pipe. The volume of fog which is illuminated and visible through the visor is 10 l. The fog has a dull white appearance, while crystals scintillate brilliantly and are easily visible against a background of matt black paint. Black velvet is a better background in the laboratory but cannot be used in the aircraft as it gives rise to excessive quantities of dust.

3. Operation of the equipment in flight

A sample of the air from outside the aircraft is injected into the observation chamber through a hose, 4 cm in diameter, one end of which projects forward from the nose of the aircraft, the other being inserted through the visor hole. The box is flushed with outside air for not less than $\frac{1}{2}$ min, the rate of air flow being 2,000 l/min; this is equivalent to a change of air in 2 sec or 150 m of aircraft travel. There are two reasons for flushing rather than for inserting a sample of outside air, e.g., with a syringe. It is necessary to remove the air previously in the chamber lest any nuclei it contains should interfere with observation of those in the fresh sample (it is found that if the observation chamber contains a large number of ice crystals formed on silver-iodide nuclei, the number is reduced

by a factor of about 10^4 by flushing with clean air for $\frac{1}{2}$ min). Secondly, it is likely that measurements of very small concentrations of freezing nuclei will be required and it is therefore desirable that the sample of outside air should not be diluted before measurements are made.

Having obtained the sample of outside air, the observation chamber is cooled down at a steady rate of about 4°C/min (see Section 5) to − 33°C, below which the rate of cooling becomes progressively less, falling to zero at about − 43°C. A fog is maintained in the chamber as it cools. The operator attempts to provide a fog dense enough to be easily visible, but transparent enough to afford easy visibility of ice crystals. The maintenance of a constant supply of fog necessitates the aircraft's altitude remaining constant within $\pm$ 20 m. This is possible in smooth air but not in bumpy conditions. When greater changes of height occur the fog tends to be supplied in puffs and the accuracy of observation is reduced.

The number of ice crystals which appear is estimated by visual inspection (see Section 4) and recorded as a function of temperature. When the lowest temperature required has been reached, the observation chamber is rapidly warmed and maintained at a temperature above 0°C for about 2 min in order to melt any frost on the walls of the chamber, ensuring that none of the incoming air is cooled below its frost point or dew point in the presence of this frost. A fresh sample of air is then injected into the chamber and the cycle is repeated. Four to seven measurements per hour can be made when the equipment is used in this way.

There is a temperature inversion at the top of the chamber: fog stays below the inversion and very little mixing appears to take place between the air below and above it. On some flights the visor hole was sealed with a sheet of glass immediately flushing ceased, to prevent contamination of the air sample, but this had no detectable effect. There is appreciable convection in the chamber due to the changing temperature of the walls and to the supply of fog, local air velocities appearing to be of the order of 1 to 5 cm/sec.

The temperature recorded is that of the lower thermistor. The upper one records a temperature 1°C higher when the temperature is steady, and 3°C higher when cooling at 4°C/min.

4. Observation of numbers of crystals

Many experiments have been described in which ice crystals formed, in cold chambers, on silver-iodide and other nuclei. Two methods have been used for determining the concentration of nuclei. In one, the ice crystals fall on to microscope slides at the bottom of the chamber and are counted; in the other, the concentration of crystals in suspension is estimated by visual inspection. The first method provides absolute measurements but is too slow for use in aircraft, so visual estimation must be used. The authors estimate the spacing between crystals and record the equivalent concentration to the nearest factor of three.

These visual estimates of crystal concentration bear a simple relation to the total concentration of freezing nuclei only in carefully controlled conditions, a point which seems to have been neglected in some previous investigations. The relationship was therefore investigated in the laboratory. The ratio of the total concentration of freezing nuclei to the estimated concentration of ice crystals is hereafter called the conversion factor. It is, in general, greater than unity, because ice crystals form on the nuclei after an appreciable lapse of time (Vonnegut 1949) and the rate of fall of ice crystals, once formed, is such that those which appear first settle out before others have appeared. Experiments to determine the value of the conversion factor are described below. For these tests a

source of freezing nuclei was required, and silver-iodide smoke was used because it is very easy to produce, and also because in some of the experiments for which the equipment is designed, the nuclei which are to be detected consist of silver iodide. Some of these tests may need to be repeated when more is known about the nature of the freezing nuclei in the free atmosphere.

The observation chamber was maintained at a steady temperature and a sample of silver-iodide smoke, produced by the kerosene burner described in Section 8 below, was injected into it from a syringe. Ice crystals appeared and their concentration was estimated at intervals until it had fallen to a low value. Meanwhile, slides were exposed in the bottom of the chamber and the crystals which fell on them were counted. The results of a typical experiment at $-16°C$ are given in Fig. 2. The estimates of concentration decrease fairly regularly with time, and a concentration of $10^{2·8}$ crystals/l is indicated after 1 min, the earliest time when the crystals were distributed throughout the chamber. The number of crystals which fell on microscope slides, however, indicated a total nucleus concentration of $10^{3·8}$/l, or ten times the value estimated.

Similar experiments were performed at other temperatures: the results are illustrated in Fig. 3. The conversion factor varied with temperature, being 30 at $-9°C$, 10 at $-17°C$ and 3 at $-26°C$. At these temperatures, the times taken for the estimated crystals concentration to decrease by a factor of 10 were 9, 4 and 1·5 min respectively. Thus at lower temperatures, at which ice crystals form more rapidly, the number of crystals visible at one time represents a larger proportion of the total number of nuclei present.

The upper limit to the concentration of ice crystals which can be reliably estimated with this apparatus is about 10^5 crystals/l. Water then diffuses very rapidly to the ice crystals and it is difficult to maintain any surplus fog. For this reason the estimated concentration of crystals never exceeds 10^6/l. The lower threshold to the detectable ice-crystal concentration is set by the dimensions of the observation chamber. With 1 crystal/10 l there is, on the average, 1 crystal in the beam of light. The lowest concentration of freezing nuclei which can be detected depends on the conversion factor, and so varies with temperature. The lower limit for silver iodide is 3/l at $-9°C$, 1/l at $-17°C$ and 1/3 l at $-26°C$.

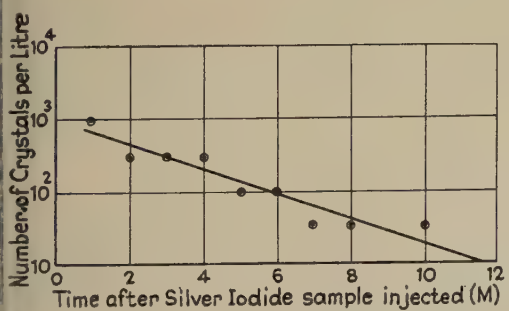

Figure 2. Sample time-variation of visually estimated ice-crystal concentration. Temperature $-16°C$.

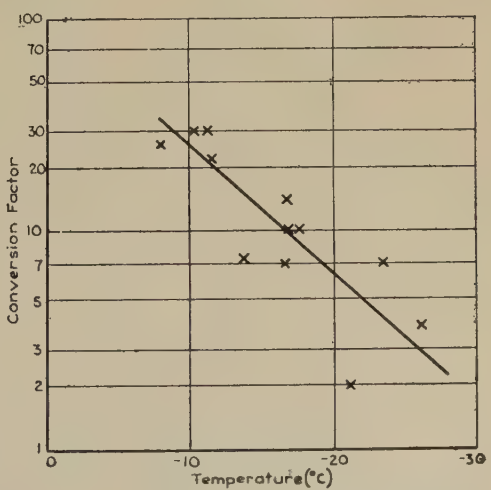

Figure 3. Conversion factor from total number of freezing nuclei to number of ice crystals observed at one time. Variation with temperature.

5. Rate of cooling

Estimates are required, in the shortest possible time, of the concentration of the freezing nuclei which are effective at various temperatures. With this apparatus, observations can be made more quickly while the temperature falls steadily than at a series of steady temperatures, but both the temperature readings and the estimated crystal concentrations are then affected by various time lags. Tests were therefore carried out to determine the effect of rate of cooling on the estimated crystal concentration at various temperatures. Samples of silver-iodide smoke, as nearly identical as possible, were prepared with the kerosene burner, introduced into the observation chamber with a syringe, and cooled at various rates.

The results of the tests are displayed in Fig. 4. The full curve, and the points, refer to experiments in which smoke samples were injected into the chamber maintained at a steady temperature. The number of ice crystals observed per gram of silver iodide increased with decrease of temperature to $-18°C$ but increased only slowly below this temperature.

The two dotted curves in Fig. 4 refer to experiments in which the smoke sample was introduced into the observation chamber and cooled at steady rates of 5°C/min and 1·25°C/min respectively. Each curve represents the mean of several experiments. These two curves differ from the full curve in two respects. At temperatures around $-8°C$, the recorded temperature at which a given concentration of ice crystals was observed was higher when the cooling rate was greater, suggesting that the time constant of the thermometer was greater than the time taken for crystals to form and grow to visible size. At temperatures below $-15°C$ the estimated concentration of crystals

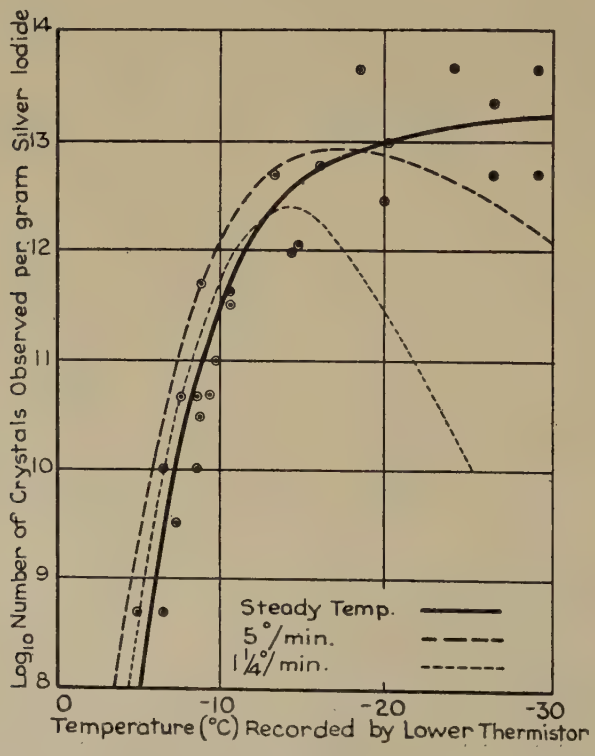

Figure 4. Number of ice crystals observed per gram of silver iodide at various rates of cooling. Points refer to steady temperature.

decreased with decrease of temperature, the rate of decrease being greater for the lower rate of cooling. This appears to be due to crystals, formed at a certain temperature, falling out of the fog in the time taken to reduce the temperature to some lower value, at a greater rate than that at which new ones are formed. It does not necessarily indicate any change in the number of effective nuclei. Tests were made of the effect of the rate of cooling at lower temperatures by cooling to a temperature at which about 10^6 ice crystals/l appeared on natural nuclei. On the day when this test was done, this took place at an indicated temperature of -37°C when cooling at 1°C/min and -36°C when cooling at 3°C/min, the maximum rate available at that temperature.

The greatest rate at which the operator can conveniently control the equipment, observe and record the results corresponds to a cooling of 4°C/min. It is concluded that this is the most suitable cooling rate at which to operate in routine investigations, and that 1° should be subtracted from the observed temperatures to allow for the lag. In cases where the estimated concentration of ice crystals decreases with decreasing temperature, it is probably realistic to assume that the concentration of effective freezing nuclei is independent of temperature and is equal to the concentration effective at the (higher) temperature where the estimated concentration is a maximum.

6. Limitations of equipment

The equipment suffers from certain limitations, some of which have been discussed in previous paragraphs. They are summarized below.

(a) *Threshold of detection*

The equipment cannot detect nuclei in concentrations less than about 1/l, the figure varying with temperature and possibly with type of nucleus (Section 4). The nucleus concentration necessary for the formation of rain has been estimated as anything from 1/10 l (Ludlam 1952) to 10 or 100 per litre (Workman 1950). A fair average estimate seems to be 1/l. If fragmentation of ice crystals takes place during growth, the required concentration of primary nuclei would be still lower. It is not therefore clear whether the detection threshold is low enough.

(b) *Method of measuring concentrations*

The method of determining crystal concentration by visual estimation (Section 4) is subjective and not very satisfactory. It is thought to be sufficiently accurate for initial observations, but phenomena of special interest may require investigation by some better method. This would have to be specially developed for use in aircraft, preferably without materially increasing the time taken to obtain observations.

(c) *Failure to detect pure-ice nuclei*

In some circumstances there may be in the atmosphere freezing nuclei consisting of small particles of pure ice, possibly formed by splintering during the growth of crystals. They could be of meteorological importance if, for example, they fell through an atmosphere saturated with respect to ice into a supercooled water cloud. They would not be detected by the equipment as they would be melted when introduced into the warm observation chamber.

(d) *Performance in clouds*

A possible source of error would arise if the equipment were used in supercooled clouds. The air intake might become coated with ice, pieces of which might find their way into the observation chamber and, being of appreciable size, survive until the temperature fell below freezing. Even if they melted, any freezing nuclei which they contained could affect the accuracy of measurement. This cause of error could easily be cured by fitting an air-intake heater to prevent the formation of ice.

(e) *Alteration of properties of nuclei*

It is possible that the properties of some freezing nuclei may depend on the time they have been at a given temperature. If this is so, the equipment might give inaccurate results due to the heating and comparatively fast cooling which the nuclei undergo.

(f) *Slow rate of operation*

The equipment takes from 9 to 15 min to make a measurement, depending on the temperature range covered (Section 3). This is fast enough for many investigations, but is undesirably slow for plotting three-dimensional contours of nucleus density around a silver-iodide burner, where it is necessary to complete the measurement before there is an appreciable change in wind or other relevant conditions.

7. Observations in flight of natural freezing nuclei

Between Nov. 1952 and July 1953, 31 flights were made in the neighbourhood of Sydney, N.S.W., in which 155 individual air samples were taken at various altitudes, and the concentration of ice crystals which formed in them at various temperatures was determined. The altitudes at which air samples were taken varied from 20 m to 5,000 m and the ambient temperatures from $-5°C$ to $+20°C$. Flights were made in a wide variety of weather conditions.

(a) *Relation between freezing-nucleus concentration and temperature*

Four typical curves showing the concentration of ice crystals which formed at various temperatures are given in Fig. 5. In curve A (13 Feb. 1953, 300 m altitude) no ice crystals appeared until the temperature fell below $-30°C$, after which the concentration increased to 10^6/l (complete glaciation) at $-37°C$. Curves B and B′ (5 May 1953, 2,000 m and 12 June 1953, 4,000 m respectively) illustrate occasions when ice crystals appeared at temperatures of $-10°C$ to $-25°C$; the concentration rapidly increased to between 10/l and 10^4/l, and then remained fairly steady until the temperature fell below $-32°C$. At lower temperatures, the concentration again rapidly increased to 10^6/l at about $-36°C$. Curve C (21 Aug. 1953, 1,500 m) is similar to curve B except that a few ice crystals (1/3 l) formed at $-11°C$. It appears that curve A represents a type of nucleus (as defined in Section 1) which is always present in large numbers. Curves B and B′ represent a second type of nucleus which is usually present in variable quantities and with somewhat variable characteristics, and curve C represents a third type of nucleus which is sometimes present, but always in small concentrations. These will be called type A, type B and type C nuclei respectively. When sufficient type B nuclei are present, it is impossible (as in curve B′) to detect the presence of type C nuclei. These three types of nuclei bear some resemblance to those previously reported in laboratory investigations. For instance, Findeisen and Schultz (1944) described two types of nuclei effective at about $-15°C$ and $-30°C$, while many observers (e.g., Cwilong (1947), Schaefer (1948)) have reported that all water drops freeze at temperatures around $-40°C$.

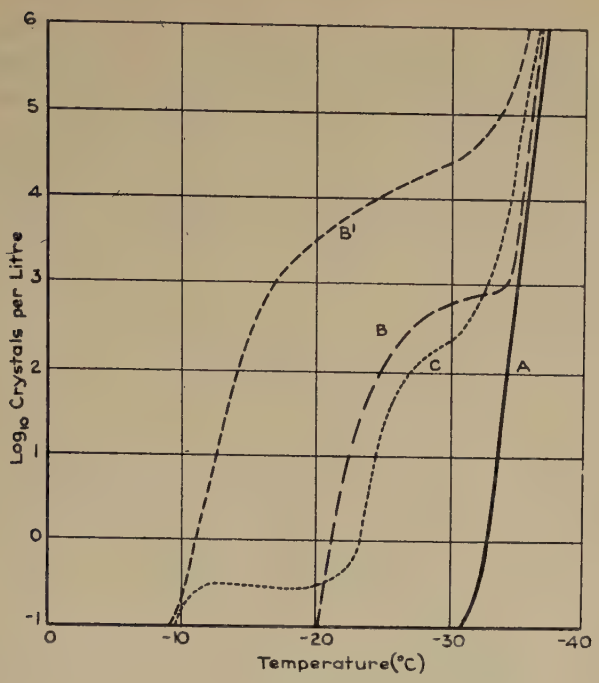

Figure 5. Typical relations between ice-crystal concentration and temperature.

(b) *Temperature of complete glaciation*

Table 1 illustrates the number of occasions on which complete glaciation (10^6 crystals/l) occurred at various temperatures.

TABLE 1. TEMPERATURE OF COMPLETE GLACIATION

Temperature (°C)	− 31	− 32	− 33	− 34	− 35	− 36	− 37
No. of occasions	2	2	2	18	47	66	17

The average temperature for complete glaciation was $-35\frac{1}{2}$°C and the average difference from this temperature was only 1°. It seems that type A nuclei are always present in adequate quantities and their properties do not vary much. It is doubtful whether this has any meteorological significance except on those few days when no freezing nuclei are present which are effective at higher temperatures.

(c) *Temperature at which 1 crystal per litre appeared*

The highest temperature at which 1 crystal/l formed is thought to be of meteorological significance (see Section 6 (a)). This usually reflects the concentration and properties of type B nuclei. The number of occasions when 1 crystal/l was first observed at various temperatures is given in Table 2. In making these measurements the air sample was cooled well below the ambient temperature at the altitude where it was collected. Thus the temperatures recorded in Table 2 correspond to the ambient temperature at considerably higher altitudes. For example, a temperature of − 15°C is characteristic, in the Sydney area, of an altitude of 5,000 m in winter and 7,500 m in summer. If the

same freezing-nucleus content was present at altitudes where the temperature was appropriate, it would have been impossible for highly supercooled clouds to form without precipitation or glaciation occurring on many occasions.

TABLE 2. HIGHEST TEMPERATURE AT WHICH 1 CRYSTAL PER LITRE APPEARED

Temperature (°C)	− 10 to − 14	− 15 to − 19	− 20 to − 24	− 25 to − 29	− 30 to − 34
No. of occasions (individual readings)	13	36	61	35	10
No. of occasions (daily averages)	1	7	14	7	1

The artificial stimulation of precipitation by silver iodide depends on increasing the number of effective freezing nuclei at a given temperature, or alternatively, on increasing the temperature at which there are sufficient effective freezing nuclei to initiate precipitation. The number of effective nuclei per gram of silver iodide is a function of temperature, being greatest at − 20°C or still lower temperatures (see Fig. 4). On days therefore when there are sufficient natural freezing nuclei effective at higher temperatures, very large quantities of silver iodide would be required to increase this temperature still further. The release of small quantities of silver iodide is then unlikely to be profitable, and the detection of small concentrations of silver iodide by its ice-nucleating properties is impossible.

(d) *Variability of individual readings*

On seven occasions, four or more successive freezing-nucleus determinations were made at the same altitude and the same ground position. The air position would change between readings by a few kilometres owing to wind movement. The highest temperatures at which 1 crystal/l appeared differed from the mean for the set of readings by 1½°C on the average. Thus large variations in the temperature at which 1 crystal/l appears occur from day to day but not from one reading to the next.

(e) *Distribution with altitude*

On 20 flights freezing-nucleus determinations were made above and below temperature inversions or isothermal layers, whose altitude varied from 1,000 m to 3,500 m. The difference in the highest temperature (average for all readings on the one flight) at which 1 crystal/l formed above and below the inversion is given in Table 3. On the average, this temperature was 3°C higher above the inversion than below it. The reason for this is not known. Freezing nuclei may originate at high altitudes, or arrive in the Sydney area with the prevailing winds at high altitudes, and diffuse downwards. Possibly some substance exists in the lower levels of the atmosphere which destroys the nuclei.

TABLE 3. DIFFERENCE IN TEMPERATURE AT WHICH 1 CRYSTAL PER LITRE FORMED ABOVE AND BELOW INVERSIONS

Temperature difference (°C)	− 4	− 3	− 2	− 1	0	1	2	3	4	5	6	7
No. of occasions	1	1	1	1	1	2	0	3	2	3	4	1

With the exception of the discontinuity at temperature inversions described above, there was no correlation between freezing-nucleus count and altitude.

(f) *Type C nuclei*

As mentioned above (Section 7 (a)), a few ice crystals sometimes appeared at temperatures around — 12°C, on nuclei which we designate ' type C.' These have frequently been observed in concentrations of 0·3/l; never more than 1/l. When present in substantially smaller quantities they would not be detected (Section 6 (a)). All that can be said of these nuclei, therefore, is whether they were detected or not. On some occasions their presence would be masked by excessive quantities of ' type B ' nuclei (Section 7 (a)). In the tables which follow the word ' doubtful ' refers to this situation.

The behaviour of these crystals is similar to that of crystals formed on silver-iodide nuclei. The suspicion naturally arose that these ice crystals formed because of contamination of the aircraft and equipment with silver iodide in the experiments described below (Section 8). However, they were not detected any more frequently directly after these experiments than at other times. Also the observations described below make this suggestion as to their origin appear most unlikely.

The number of occasions when type C nuclei were detected, not detected and ' doubtful ' is displayed in Table 4, for high and low altitudes. By definition ' high altitude ' here means above a temperature inversion or above the 1,500-m level if there was no inversion. Thus type C nuclei were detected more often at high altitudes than at low.

TABLE 4. Occurrence of type C nuclei as a function of altitude

No. of occasions	Detected	Doubtful	Not detected
High altitude	19	27	32
Low altitude	12	14	51

The occurrence of type C nuclei was correlated with the direction of the wind at the altitude at which they were observed. Table 5 shows the number of occasions when they were detected in winds blowing from various directions, at high and low altitudes as defined above. When the wind speed was less than 3 m/sec the reading was omitted.

TABLE 5. Occurrence of type C nuclei as a function of wind direction

Wind direction	High altitude			Low altitude		
Degrees true	Detected	Doubtful	Not detected	Detected	Doubtful	Not detected
0- 80	0	3	1	4	2	6
90-170	1	7	11	1	6	24
180-260	16	6	8	6	1	3
270-350	1	8	10	1	3	10

There was a pronounced tendency at high altitudes for type C nuclei to be detected only in SW winds. At low altitudes they were detected in SW and NE winds. In this area in summer and autumn, when most of the measurements were taken, it is common for north-easterly sea breezes to blow at low altitudes in the afternoon, the coast running NNE to SSW. Thus it is possible that, even at low altitudes, type C nuclei come only from the SW, drifting a few kilometres out to sea in the morning and back again in the afternoon.

(g) *Correlation with rainfall*

No noticeable correlation was observed between the occurrence of rain in the Sydney area and the number of freezing nuclei. Sufficient data are not yet available to determine whether there is a correlation in particular meteorological conditions.

8. Observations in flight of the trajectory of silver-iodide nuclei

Many investigators and commercial operators have in recent years released freezing nuclei in the form of silver-iodide smoke from generators on the ground, hoping that it will be carried up into supercooled clouds and initiate precipitation. It has however, not been established whether, where, and in what concentration, these nuclei rise to altitudes where the temperature is appropriate, and there is evidence (e.g., Reynolds, Hume, Vonnegut and Schaefer 1951) that they are decomposed by the action of sunlight. Investigations have been commenced with the equipment described above, of freezing-nucleus concentration as a function of position relative to a silver-iodide smoke generator. Some preliminary results are described below.

(a) *Silver-iodide smoke generators*

Two generators have been used, with hydrogen and kerosene flames respectively. The hydrogen burner has been described and illustrated by Vonnegut (1949). An acetone solution of silver iodide is atomized in a spray gun, using hydrogen at a pressure of $2\frac{1}{2}$ atmospheres. The mixture of hydrogen and silver-iodide solution spray is burnt, a flame tube being used to prevent the flame blowing out. The rate of consumption is 1 l of solution, containing 200 g of silver iodide, and 6,000 l of hydrogen (at atmospheric pressure) per hour. In the kerosene burner, the silver-iodide solution is atomized in the same way, using compressed air at $2\frac{1}{2}$ atmospheres. The spray is directed, from a range of 10 cm, into the flame of a large kerosene blowlamp which burns 2 l of kerosene per hour. The silver-iodide solution is prepared by dissolving one part by weight of sodium iodide in five parts of acetone, dissolving in this solution four parts by weight of silver iodide, filtering and adding ten parts by weight of acetone.

The number of freezing nuclei per gram of silver iodide produced by these burners was determined in the manner described by Vonnegut (1949). The burner was operated in one end of a tube, 1·2 m square and 7 m long. An electric fan in the tube sucked the smoke down the tube, thus diluting the smoke and thoroughly mixing it with a large known volume of air. Samples of dilute smoke were taken from the far end of the tube by a syringe and further diluted by expelling $\frac{9}{10}$ of the sample from the syringe and replacing with clean air, as often as required. Smoke from the syringe was then introduced into the observation chamber which was maintained at a steady temperature, and the number of ice crystals which formed was observed. This was repeated at various temperatures. The number of freezing nuclei/g was determined from the estimated number of ice crystals/g as described in Section 4. Less than 2 min elapsed between burning the silver iodide and injecting it into the observation chamber. The concentration of freezing nuclei in the syringe decreased, owing to precipitation on the walls, by a factor of ten in a few hours, so errors due to this cause in measurements of concentration should be negligible. The number of nuclei/g produced by the kerosene burner was 10^{11} effective at -7°C, 10^{13} at -11°C and 6×10^{13} at -20°C to -30°C. The hydrogen burner produced 10^{12} nuclei/g effective at -7°C, 10^{14} at -11°C and 2×10^{15} at -20°C to -30°C.

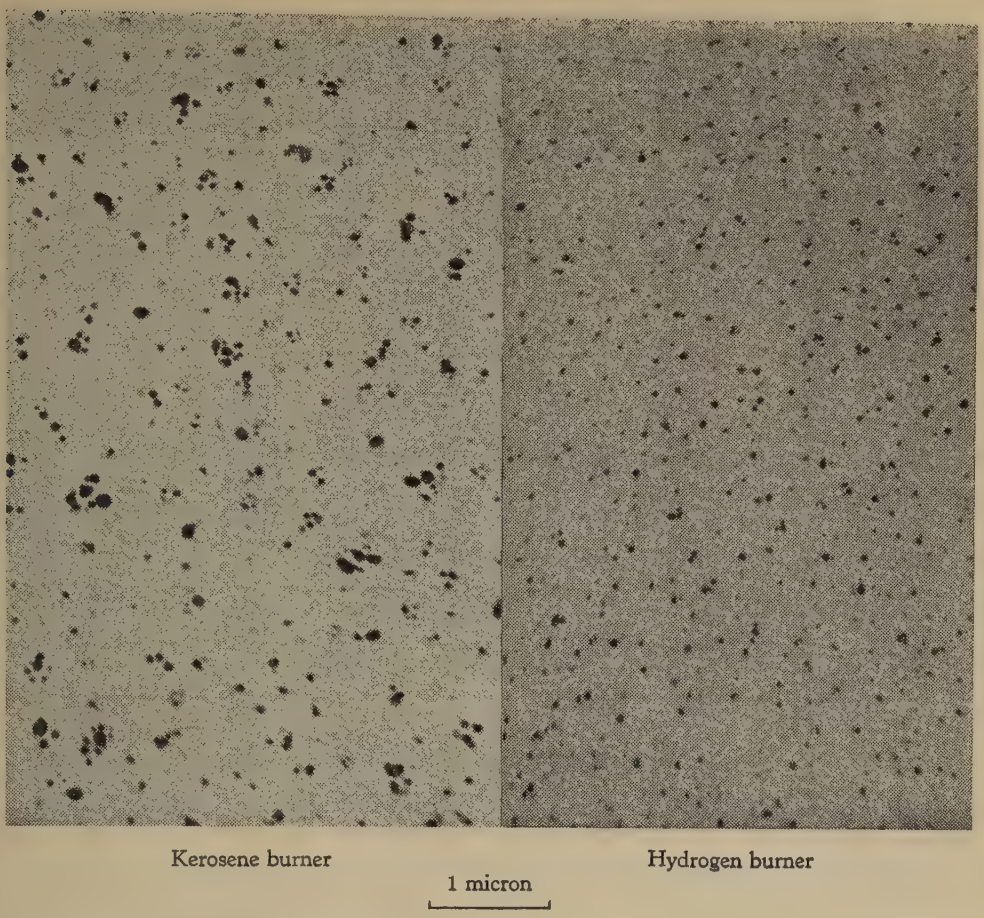

Figure 6. Electron photomicrographs of silver-iodide smoke particles.

Electron photomicrographs of samples of the smoke produced by the two burners are shown in Fig. 6. The smoke particles were collected on a collodion film on a wire mesh by moving it in and out of the smoke for a few minutes, about 1 m from the generator, as described by Vonnegut (1949). The kerosene burner produced larger particles than the hydrogen burner, and some of them appear on the photograph in clusters, suggesting that some drops of solution were not evaporated or burnt.

(b) *Flying plan*

The rate of measurement of freezing-nucleus concentration by this equipment is too slow to enable three-dimensional investigations to be made in acceptable detail in a reasonable time. Consequently, one flight has been devoted to measurements at one distance from the smoke generator, at a number of heights and a number of bearings relative to the wind. When collecting a sample the aircraft flew, at constant height, directly towards the smoke generator on a track at the required angle to the wind. The observation chamber was flushed with outside air for one minute, flushing being stopped when the aircraft reached the predetermined distance. The aircraft then flew outside the smoke plume at constant height while the air sample was analysed for freezing-nucleus content. The process was then repeated at another heading or altitude. Progressively increasing altitudes were

explored until a height was reached where no smoke could be detected: the horizontal limits of the smoke were also investigated. Periodically, air samples were taken, upwind from the generator, to determine the concentration of freezing nuclei in the absence of silver iodide. No tests were possible on days when many natural freezing nuclei, effective at temperatures higher than — 20°C, were present. (See Section 7).

Wind measurements were made by pilot balloons, and in addition, balloons fitted with long paper tails and weighted to give a low rate of rise were periodically released. These assisted the pilots in finding the smoke, which is invisible at distances exceeding a few metres. Most of the flights took place at Dubbo, N.S.W., 250 km NW of Sydney, where the terrain is flat.

(c) *Weather conditions*

Tests were made only on days with simple weather situations, with steady winds which were almost constant with time and height throughout the period of a flight. Wind speed varied from 2 to 8 m/sec with an average of 4 m/sec. Lapse rates varied from 7·3°C to 9·9°C per 1,000 m, with an average of 9·4°C per 1,000 m. They were substantially uniform throughout the altitude range where smoke was detected. The only temperature inversions which occurred were at much higher altitudes. Cloud cover varied from nil to complete. No precipitation fell during any test. Relative humidities at ground level varied from 25 to 60 per cent and, at the highest altitudes at which silver iodide was detected, from 35 to 70 per cent.

(d) *Results of individual flight, 6 May* 1953

On this occasion winds were easterly at 2 m/sec at all relevant altitudes. There were 2/8 altocumulus at about 1,500 m altitude. The lapse rate was 7·6°C per 1,000 m. The kerosene burner was used, and measurements were made at a distance of 5 km.

The results of an individual measurement are illustrated in Fig. 7, in which estimated ice-crystal concentration is plotted against temperature, which fell at 4°C/min. The full curve represents an air sample taken upwind from the smoke generator, and is a measure of the natural freezing-nucleus content of the air. The dotted curve represents an

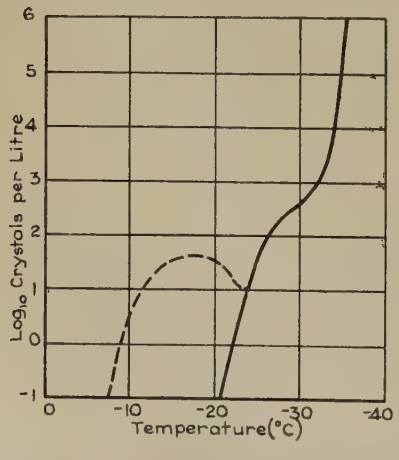

Figure 7. Ice-crystal concentration in relation to temperature. Full curve, upwind from silver-iodide smoke generator. Dotted curve 5 km downwind, 300 m altitude.

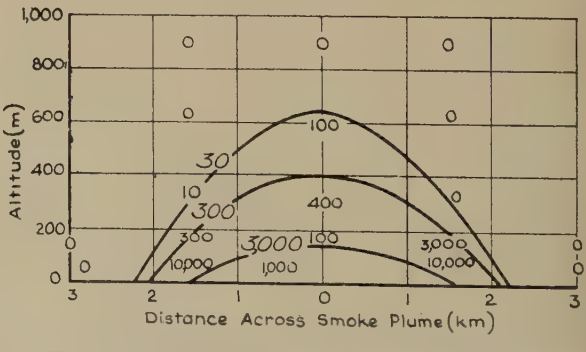

Figure 8. Cross-section of smoke plume at 5 km from burner. Concentration of freezing nuclei per litre.

air sample, taken at 300 m altitude, directly downwind from the smoke generator. Ice crystals started forming on silver iodide nuclei at — 8°C, the estimated concentration increasing as the temperature was reduced to — 15°C. It then remained constant until the temperature reached — 20°C, decreasing at lower temperatures owing to crystals falling out of the fog at a faster rate than that at which new ones formed. (See Section 5). At — 24°C the dotted curve merges with the full one, as ice crystals formed on natural nuclei at lower temperatures. The concentration of ice crystals at the peak of the curve at — 17°C, multiplied by the appropriate conversion factor of ten (see Section 4), is taken to represent the freezing-nucleus concentration due to silver iodide at that distance and altitude. In this case it was 400 nuclei/l.

Nineteen similar series of measurements were made at various altitudes and bearings from the smoke generator. In addition, three were made upwind from the generator, no ice crystals forming at temperatures above — 20°C. The measurements made downwind from the smoke generator are displayed in Fig. 8. The ordinates represent altitude above the ground and the abscissae, distance across-wind from a point 5 km downwind from the smoke generator. The figures represent the concentrations of freezing nuclei measured at the altitude and position indicated. They are not particularly consistent, no doubt owing to short-period meanderings of the plume of smoke; 3½ hr elapsed between the first and last measurements. The lateral positions of some of the measurements have been adjusted to bring the measured centres of the smoke plume at all heights into one vertical line. These adjustments did not exceed 1 km.

Through these points tentative isopleths are drawn, which are intended to represent the envelopes of concentration at this distance, averaged over a considerable period of time. The instantaneous concentrations would be higher over a smaller area. The concentration and also the plume width were greatest near the ground.

From these isopleths and the observed wind velocity the flux of effective freezing nuclei at this distance from the smoke generator was computed. In this case it was 10^{16} nuclei/hr, which does not differ significantly from the production of the smoke generator.

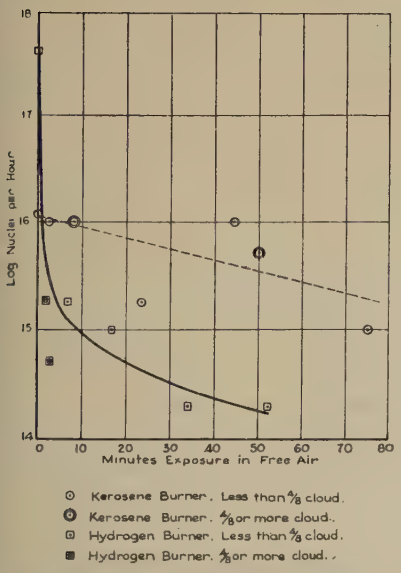

Figure 9. Flux of freezing nuclei in relation to time of exposure in free air.

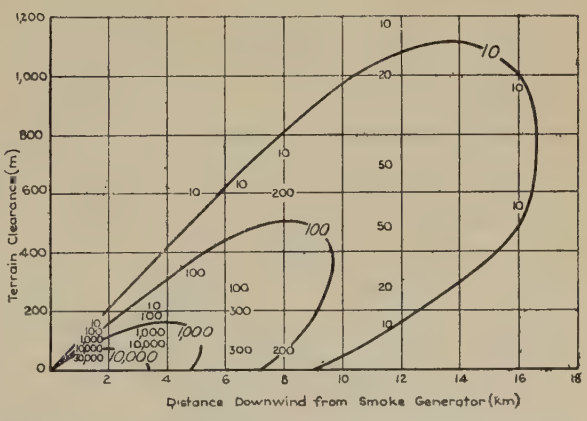

Figure 10. Distribution of freezing nuclei in vertical plane downwind from silver-iodide smoke generator (kerosene burner).

(e) *Results of several flights*

Between Jan. 1953 and July 1953, 13 similar flights were made. The kerosene burner was used in seven flights, the hydrogen burner in the other six. The measurements of flux of freezing nuclei effective at − 17°C are displayed in Fig. 9, plotted against the time taken for smoke particles to drift with the prevailing wind from the burner to the distance where they were measured. In the case of the hydrogen burner, the flux decreased very rapidly during the first 2 min, then more slowly. After 25 min exposure the flux had decreased to one-thousandth of the production of the burner. With the kerosene burner the flux decreased much less rapidly, the accuracy of the measurements being barely sufficient to detect so small a decrease. The flux of effective nuclei from the kerosene burner exceeded that from the hydrogen burner after an exposure time of 1 to 2 min. The effects of cloud cover do not appear to be significant, although in two cases (hydrogen burner, exposure times 2 or 3 min) the sky was completely covered with altocumulus.

The dispersion of the smoke in the vertical plane is illustrated in Fig. 10, for the kerosene burner. The figures are numbers of effective freezing nuclei per litre, at the altitudes and distances from the smoke generator indicated respectively by the ordinates and abscissae. They have been converted to a wind speed of 5 m/sec by assuming that concentration is inversely proportional to wind speed. These figures are taken from the isopleths of the plots of individual measurements, similar to Fig. 8. Thus they represent the average of several individual readings over a considerable period of time. Isopleths are drawn on Fig. 10 representing the zones inside which the concentration of freezing nuclei effective at − 17°C exceeds the value stated.

The maximum concentration of nuclei occurred close to the ground at distances up to about 4 km, while at greater distances the concentration was greatest at some higher altitude.

Vertical dispersion with the hydrogen burner was similar to that shown in Fig. 9. However, owing to the decrease in number of effective nuclei after exposure in the free air mentioned above, the maximum distance at which 10 nuclei/l, effective at − 17°C was observed, was 7 km, 100 nuclei/l at 4 km and 1,000/l at 2 km. The plume width, with both burners, was such that most of the smoke particles were contained within a width of approximately two-thirds the distance from the generator.

9. Comments

The use of a cold chamber in an aircraft has been shown to be practicable for measurements of the concentration of natural and artificial freezing nuclei in the free atmosphere.

The preliminary observations of natural freezing nuclei show marked variations from day to day in the concentration of ice crystals which form at a given temperature in a supercooled cloud. The nature of the nuclei on which they form, and the correlation with meteorological phenomena, have yet to be established.

The results of the silver-iodide trajectory measurements have many interesting applications to the field of rainmaking. For example, the rapid decay in effectiveness of the silver-iodide freezing nuclei, detected in the laboratory (e.g., by Reynolds *et al.* 1951) is confirmed in the free atmosphere in the case of the hydrogen burner. The rate of decay of nuclei from the kerosene burner is much less, but this burner produces fewer nuclei per gram of silver iodide. Thus if for practical rainmaking purposes it were required that large numbers of durable freezing nuclei be produced, then as far as can be judged from these two burners, the amount of silver iodide required would be very large. The

reason for the difference in durability between nuclei produced by the two burners is not known but it may well be connected with the larger size of those from the kerosene burner.

It is not likely that the measurements of dispersion of silver iodide can safely be extrapolated to longer distances and greater heights. Braham, Seely and Crozier (1952) have measured the dispersion of an aerosol of small zinc-sulphide particles. These dispersed in a similar manner to that described above for silver iodide in the first 20 km of drift downwind, but dispersion thereafter, both laterally and vertically, was much less rapid, the average concentration decreasing by a factor of only three between the ranges of 16 and 160 km from the dispenser. Silver iodide may well behave in a similar manner. The dispersion measurements reported above do not rule out the possibility that some silver-iodide particles were transported by local updraughts to greater heights than those at which they were detected.

Acknowledgments

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Evaporation from an oat field

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SUMMARY

Evaporation from natural surfaces may be evaluated by a method based on aerodynamic considerations which lead to the simple equation

$$E = \frac{\rho k^2 (u_2 - u_1)(q_1 - q_2)}{\left[\ln\left(\frac{z_2 - d}{z_1 - d}\right)\right]^2}$$

in which E, ρ and k are the rate of evaporation, the air density and von Kármán's constant respectively, u_1, u_2, q_1 and q_2 are the wind speeds and specific humidities at the heights z_1 and z_2, and d the so-called surface zero displacement. This formula has been applied to observations taken over an oat field in order to determine the hourly values of E during a number of periods ranging in length from one to twelve days. The magnitude of d is shown to increase with increasing crop height and wind speed, the crop height increasing eightfold over the total period of observation. Daily water losses are calculated and found to vary from 0·05 mm to 5·65 mm. By using two sets of apparatus, the calculated values of E are shown, in unstable conditions, to depend on the height of exposure of the apparatus, decreasing with increasing height. A modified form of the equation, based on Deacon's generalized wind-profile law, has been used in the last period of observation and found to reduce the dependence of the calculated values of E on the level at which the apparatus was placed. The general application of the modified expression was found to be impossible owing to insufficient precision in the measurements of the wind speeds and to the uncertainty in the value to be assigned to d. In general, the computed daily water losses are considered to be underestimated to an extent not exceeding 15 per cent.

1. Introduction

While the direct measurement of evaporation from natural land surfaces by the use of evaporimeters of simple design may be undertaken in certain favourable conditions (see e.g., Pasquill 1949a), the application of such a method to an area of tall crops is not possible. We are therefore obliged to use an indirect method, two being available at present. The first is the heat balance approach favoured by Penman (1948) which depends essentially on the performance of a conventional open water-tank evaporimeter. The second method, the aerodynamic approach, depends upon the imperfectly understood diffusive properties of the atmosphere and has been investigated in this country by Pasquill (1949a).

The observational programme to be described in this paper is an attempt to apply the aerodynamic method to the determination of natural evaporation from an oat field during the period over which the crop grew from a height of 10 cm to 80 cm and was finally reaped. Observations were not made continuously throughout the whole time but the hourly rates of evaporation in a number of periods ranging from 1 to 12 days were evaluated.

2. The aerodynamic method

The method depends upon the validity of three propositions. These are that in the normally turbulent atmosphere near the ground :

(a) the wind speed u_z at the height z ($z > z_0$) may be expressed as

$$u_z = \frac{1}{k}\left(\frac{\tau_0}{\rho}\right)^{\frac{1}{2}} \ln\left(\frac{z - d}{z_0}\right) \quad . \quad . \quad . \quad . \quad (1)$$

where τ_0 is the shearing stress at the surface, ρ is the air density, z_0 is a constant, for each surface, known as the roughness parameter, d is the surface zero displacement and k is von Kármán's constant.

(b) the shearing stress is constant with height.

(c) the eddy diffusivity for momentum is identical with that for water vapour.

Deacon (1949, 1953) has demonstrated that the functional form of Eq. (1) holds over natural surfaces in the open near the ground when the atmosphere is in the adiabatic state. In these conditions Sheppard (1947), Pasquill (1950b) and Rider (1954), have established the explicit truth of the equation. The constancy of the shearing stress with height was first proved by Ertel (1933) and has been confirmed by Calder (1939) but experimental results obtained by Scrase (1930) are not in agreement with this concept. However Pasquill (1950b) has advanced a possible explanation and Deacon (private communication) now appears to have resolved the difficulty. The identity of the eddy diffusivities for momentum and vapour has been the subject of experimental investigation by Pasquill (1949a), Rider and Robinson (1951) and Rider (1954) and the evidence for this identity appears to be fairly conclusive. In any event many expressions for diffusion in the atmosphere have been based on the validity of (a), (b) and (c) above and have been found to be in good agreement with observation. We may particularly note that Pasquill (1950a) has applied the aerodynamic method to the determination of evaporation from a clayland pasture in effectively neutral conditions and his results were in agreement with the evaporation determined directly by the use of simple evaporimeters.

If steady horizontal conditions are assumed, the three propositions given above lead to the equation

$$E = \frac{\rho k^2 (q_1 - q_2)(u_2 - u_1)}{\left[\ln\left(\frac{z_2 - d}{z_1 - d}\right)\right]^2} \quad . \quad . \quad . \quad (2)$$

where E is the rate of evaporation, and q the specific humidity, the suffixes 1 and 2 referring to measurements at the two heights z_1 and z_2. Since Eq. (1) is only applicable in neutral conditions, Eq. (2) is also subject to this restriction. The general use of Eq. (2) without regard to the state of stability of the atmosphere must lead to errors in E since the layer of air near the ground is in the adiabatic state for a few short periods only during the average day. However, it has been found that the magnitude of the Richardson Number, defined by

$$Ri = \frac{g}{T} \frac{\left(\frac{\partial T}{\partial z} + \Gamma\right)}{\left(\frac{\partial u}{\partial z}\right)^2}$$

where g is the gravitational acceleration, T the absolute temperature and Γ the dry adiabatic lapse rate, is a convenient indicator of the departure of the atmosphere from neutral conditions, and, as the surface is approached, observations have shown (see e.g., Pasquill 1949a or Rider and Robinson 1951) that the magnitude of the number decreases. Thus by taking the observations close to the surface the errors are kept to a minimum. Pasquill (1949c) estimated that by taking observations in the first 50 cm above a short grass surface in fine spring weather the systematic error, in the form of an underestimation of E in unstable conditions and an overestimation of E in stable conditions, was of the order of $\pm$ 10 per cent. This investigator (1949a) has also shown that the Deacon (1949) generalized wind-profile law

$$\frac{\partial u}{\partial z} = az^{-\beta}$$

which leads to the equation

$$u_z = \frac{1}{k(1-\beta)}\left(\frac{\tau_0}{\rho}\right)^{\frac{1}{2}}\left[\left(\frac{z}{z_0}\right)^{1-\beta} - 1\right] \quad . \quad . \quad . \quad (3)$$

where a and β are constants, β taking values > 1, $= 1$ and < 1 in unstable, neutral and stable conditions respectively, provides a satisfactory basis for the modification of Eq. (2) for unstable conditions. The modified equation is

$$E = \frac{\rho k^2 (1-\beta)^2 (q_1 - q_2)(u_2 - u_1) z_0^{2(1-\beta)}}{[(z_2 - d)^{1-\beta} - (z_1 - d)^{1-\beta}]^2} \quad . \quad . \quad (4)$$

In stable conditions Rider (1954) has shown that Eq. (3) does not hold and its use would lead to an underestimation of the eddy diffusivity for momentum and hence of that for vapour. In applying the method in stable conditions of flow we are at present forced to rely on the observed decrease in the influence of stability as the surface is approached. It will be seen later that the contribution to the daily water loss by evaporation in stable periods is generally small and the error which results from the use of Eq. (1) in these conditions is usually not significant.

3. The site, apparatus and observations

An oat field of some ten acres, with no undersown crop, was available for the observations. Its shape was roughly rectangular and its limits were marked by a track to the south, by an open wire fence to the west and by a low hedge to the north and east. Apparatus was sited in this field some 80 m from the eastern boundary and was always approached along the same path which was about 50 cm wide and ran to the apparatus from the nearest point on this boundary. From the position of the apparatus there was a uniform surface of the crop for at least 150 m in an arc from SE through S, W and N to NNE. When the observations were started the crop was 10 cm high and for the rest of the season a record of its height was kept. The crop had been sown in drills about 12 cm apart, parallel to the eastern hedge, and the growth was patchy to some extent. The hut used to house the recording equipment was outside the field and below its level in a disused gravel pit.

Records of the total run of wind were obtained by the use of Sheppard (1940) type anemometers suitably modified to operate electromagnetic counters some distance from the position of exposure. The counters associated with each of three anemometers were mounted in a photographic recorder of the type described by Hartley (1951). Photographs of the counters were taken every half hour although in the final analysis only the hourly runs of wind were used. Experience with the anemometers has shown that they become unreliable at wind speeds much below 40 cm sec^{-1}.

Measurement of the humidity at two heights was made with aspirated thermocouple thermometers in housings of the same type as those used by Pasquill (1949a, b) and Rider and Robinson (1951). Since only two psychrometers were required in one set of apparatus it was possible to mount them vertically at the extremities of a hollow mast and to provide each with a set of plates to ensure that the air was drawn from the required level. In this way the apparatus was given a multidirectional characteristic and the need to rotate the mast to prevent the direct ingress of sunshine to the thermometer bulbs eradicated. The general construction of one of the masts is shown in Fig. 1. The tubes A and A′ were made to be sliding fits in tube B so that the height interval between the levels of measurement could be adjusted as required. By fitting various lengths of main supporting mast to C, the height of the centre of the apparatus could also be adjusted.

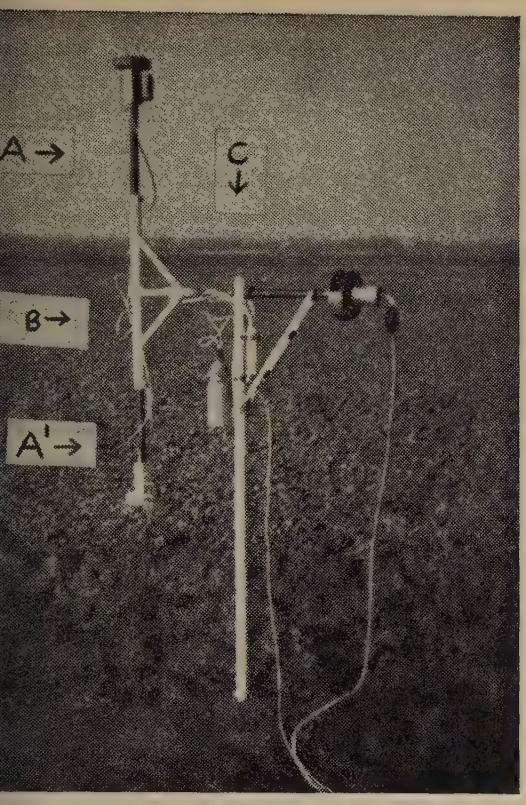

Figure 1. The humidity mast.

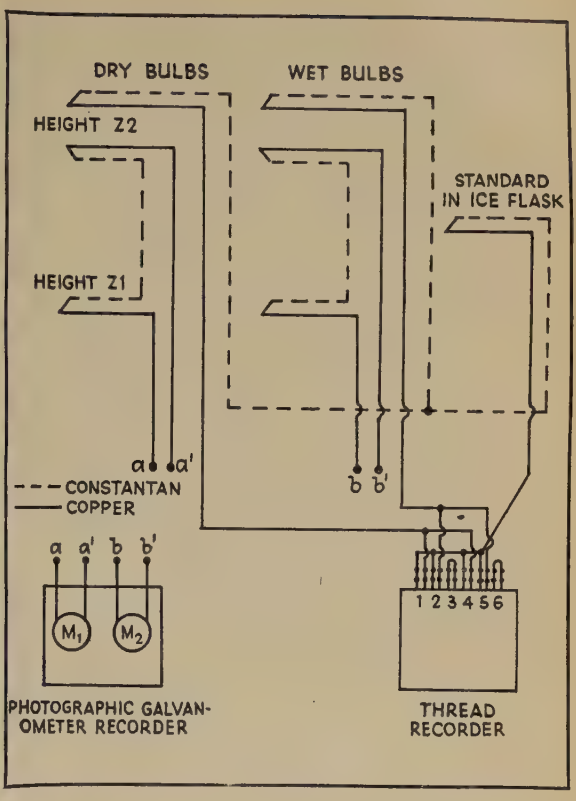

Figure 2. Wiring of humidity mast and recorders.

A vacuum flask was attached to the main support together with a junction box from which a 7-cored 22 s.w.g. single copper conductor cable led away to the hut. In use the vacuum flask was filled with melting ice and a standard junction was fitted through the cork. A wiring diagram of the apparatus is given in Fig. 2. It will be noted that the dry and wet thermometers forming the psychrometer at the higher level were each provided with two thermojunctions. One from each thermometer was connected in a standard circuit, and the other junction in each was connected in a differential circuit with the corresponding junction in the lower psychrometer. In this application, it is not necessary to know the absolute values of the dry- and wet-bulb temperatures at either height to any great accuracy, but as accurate a knowledge as possible of the dry- and wet-bulb differences between the two heights is required. The temperatures of the dry and wet bulbs at the greater height (T and T_w) were therefore recorded on a comparatively insensitive thread recorder with reference to the standard junction in the ice flask, and a photographic recorder working in conjunction with two Tinsley reflecting galvanometers provided a record of the differences in temperature of the dry and wet bulbs (δT and δT_w) at the two levels. The overall temperature sensitivity of the thread recorder when connected to the mast with 200 yd of cable was such that T and T_w could be read with difficulty to 0·1°F and are considered to be reliable to 0·2°F only. The temperature differences δT and δT_w could be obtained to 0·01°F with the photographic recorders. These limits of temperature measurement enabled the vapour pressure differences between the two levels to be calculated to 0·01 mb. The thread recorder had six circuits and switched at intervals of 2 min. Two circuits were connected in the dry-bulb circuit and two in the wet-bulb

circuit, the remaining two being used to provide a closed circuit zero. The two galvanometers in the photographic recorders gave continuous records of δT and δT_w with the qualification that every 30 min the movements were disconnected from the external circuits and short-circuited through their critical damping resistances. This was executed by a time switch and relay system, and provided a record of the zeros of the instruments as well as time marks. In all the wiring, care was taken to avoid the introduction of dissimilar metals and therefore of spurious e.m.f. The whole apparatus was subjected to lag and ventilation tests as described by Pasquill (1949b) and the lag coefficient of the dry bulbs found to be about 80 sec and that for the wet bulbs slightly less at the ventilation rate of 3 m sec^{-1}. The photographic recorders were provided with synchronous motor-driven 50-min drums for calibration purposes. It was found advantageous to place the thermometers to be calibrated together with standard mercury-in-glass thermometers in vacuum flasks about three-quarters full with water and to close the flasks with corks and shake them vigorously during calibration. The error due to the overestimation of large deflections, by the cylindrical drums in the photographic recorders, was insignificant for deflections of less than 3 cm, which corresponded to a temperature difference of 1·5°F. Temperature differences as large as this were rarely met in the series of field observations.

TABLE 1. PERIODS OF OBSERVATION AND DETAILS OF THE HEIGHTS OF EXPOSURE OF THE INSTRUMENTS

Period number	Dates (1951) and times (GMT)	Crop height cm	Apparatus set A Hts. of exposure of		Apparatus set B Hts. of exposure of		Dates and approx. times of rain or showers	
			Anemometers cm	Psychrometers cm	Anemometers cm	Psychrometers cm	Date	Time
1	25 May 13	10	25	25	Not	Not	26 May	1645 to 24
	to	to	60	60	exposed	exposed	27 May	00 to 04
	6 June 10	19	100				4 June	02 to 05
2	11 June 10	23	40	40	40	Not	11 June	20 to 22
	to	to	75	75	65	exposed	12 June	01 to 02
	16 June 10	28	115		165			05 to 08
								16 to 19
							14 June	1630 to 19
3	25 June 11	35	55	55	55	Not	26 June	00 to 0130
	to		90	90	80	exposed		07 to 10
	26 June 10		130		180			
4	2 July 15	45	70	70	95	Not	4 July	05 to 0730
	to	to	105	105	195	exposed	5 July	22 to 24
	9 July 12	61	145		245		8 July	14 to 17
5	9 July 12	61	70	70	95	Not	11 July	0830 to 10
	to	to	110	110	195	exposed	12 July	13 to 1630
	14 July 10	71	145		245			17 to 24
6	19 July 10	75	85	85	110	Not	22 July	08 to 09
	to	to	125	125	210	exposed		1530 to 17
	26 July 11	79	160		245		23 July	00 to 03
								13 to 17
								21 to 24
7	9 Aug. 09	80	85	85	110	110	9 Aug.	18 to 19
	to		125	125	210	210	11 Aug.	09 to 24
	17 Aug. 09		160		245		12 Aug.	00 to 02
								06 to 09
								12 to 13
								16 to 18
8	20 Aug. 14	Stubble	25	25	85	85	21 Aug.	20 to 22
	to	10	60	60	125	125	22 Aug.	06 to 16
	25 Aug. 10		100		160		24 Aug.	09 to 10
								16 to 17

A number of sets of apparatus was constructed and on some occasions two sets were exposed simultaneously. The apparatus was set out on occasions when fine weather was expected. The periods of recording are set out in Table 1 together with the height of exposure of the instruments, periods of rain and the heights of the crop. It will be seen that, for all periods after the first, two anemometer masts were exposed. The reason for this will appear later. A period of about 10 min was required in which to change the photographic papers and replenish the wet-bulb water holders and this amount of temperature record was lost once a day. Occasionally records were lost for short periods owing to instrument failures. The wind directions during periods 2 to 8 inclusive were within the arc which provided a fetch of at least 150 m over the oat field, with the exception of an odd hour here and there. In period 1 the wind directions were mainly easterly and the fetch was as little as 80 m at times.

The records from the anemometer recorders consisted of columns of figures which represented the number of revolutions of the cups, and the hourly runs of wind were obtained from calibration curves. The thread-recorder records consisted of the familiar series of dots and the mean height of the curves formed by the dots was a measure of $(T - 32)$ and $(T_w - 32)$°F. The determination of the temperature differences between the two dry and wet bulbs (δT and δT_w) was carried out with the aid of a planimeter. The hourly rates were divided by the drum rate, which was indicated by the automatic time-marking device, and the mean hourly ordinates obtained. Recourse to the calibration factors provided the required values of δT and δT_w.

4. The evaluation of the rate of evaporation

Before any computations of the rates of evaporation could be made it was necessary to obtain values of d, the zero displacement. Two methods are available, the first consisting of the graphical solution of the following expression which is derived from Eq. (1)

$$\frac{u_3 - u_2}{u_2 - u_1} = \frac{\ln (z_3 - d) - \ln (z_2 - d)}{\ln (z_2 - d) - \ln (z_1 - d)}$$

Alternatively we may plot observed values of u_z against $\ln (z - d)$ using likely values of d. When the best approach to a straight line is obtained we have the required value of d. When the first method was tried it soon became apparent that considerable scatter was arising in the results, presumably due to instrumental errors in the wind-speed measurements. A method in which graphical smoothing could be applied was thought necessary and so the longer method was adopted. Wind-speed measurements at three heights are not sufficient for the critical application of the second method and consequently, after the first period of observations, two anemometer masts were always exposed so that wind-speed determinations at six positions were available. The zero displacement can be obtained from observations in neutral conditions in which Eq. (1) holds and it was necessary to select the hourly periods in which this state was approached. The condition finally decided upon was that the dry-bulb temperature gradient $\partial T/\partial z$ should not exceed $\pm$ 0·001°F cm^{-1} at the mean logarithmic height of z_1 and z_2, assuming a logarithmic variation of temperature with height. Strictly the criterion of the suitability of an hourly observation for this purpose is the value of the Richardson Number but, to restrict the analytical work to reasonable proportions, the above condition was adopted as an acceptable approximation. On some days two or more values for d were obtained, but there were a few days on which no period of an hour was suitable. Work, on the form of the wind profile over long grass, by Deacon, which has been discussed by Calder (1949), indicates that d varies with the crop height and wind speed, and this feature soon became apparent

in the present observations. The determinations of d were therefore grouped according to the wind speed at 2 m, the latter being estimated from the plots of u_z against ln $(z - d)$. The groups adopted were for wind speeds at 2 m of < 1, 1 to 2, 2 to 3, 3 to 4, and > 4 m sec^{-1}. This feature provided an additional complication in the evaluation of E, it being necessary to use a value of d appropriate to the wind speed and crop height. A plot of the values of d, divided according to the wind speed groups given above, is shown in Fig. 3 in which a smooth curve has been constructed for each group. The values of d used in the computations of E from Eq. (2) were taken from these smooth curves. It will be seen that d increases with both wind speed and crop height. It was also noticed that there was a tendency for d to vary with wind direction such that when the wind was directed along the drill lines the value was often smaller than when the wind direction was at right angles to these lines. The observations were not sufficiently numerous to permit any indication of the magnitude of this effect and it has been neglected, the results plotted in Fig. 3 being for all wind directions experienced.

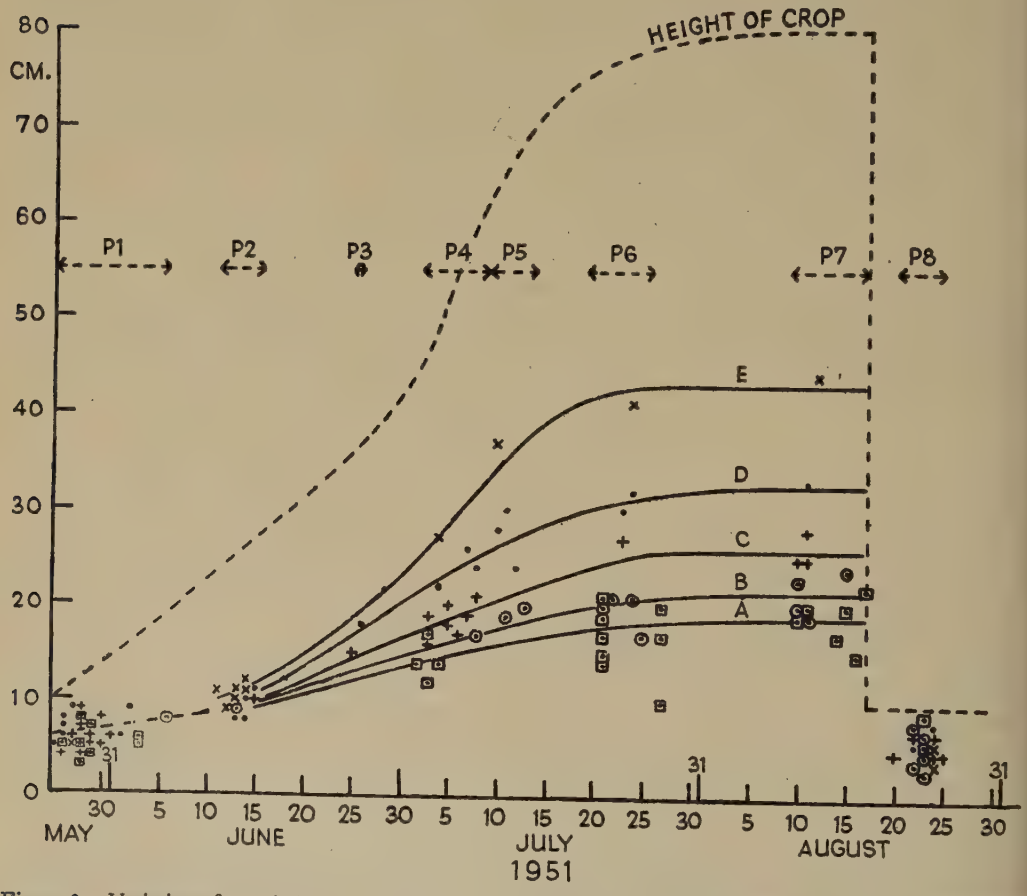

Figure 3. Variation of crop height with time (– – – –) and values of the zero displacement divided according to wind speed groups (————).

Curve A and ⊡ values of d for wind speeds at 2 m < 1 m sec^{-1}
Curve B and ⊙ values of d for wind speeds at 2 m of 1 to 2 m sec^{-1}
Curve C and + values of d for wind speeds at 2 m of 2 to 3 m sec^{-1}
Curve D and • values of d for wind speeds at 2 m of 3 to 4 m sec^{-1}
Curve E and × values of d for wind speeds at 2 m > 4 m sec^{-1}

Eq. (2) may be written as

$$E = \frac{3{\cdot}46 \times 10^{-5}\,(e_1 - e_2)\,(u_2 - u_1)}{T\left[\ln\left(\frac{z_2 - d}{z_1 - d}\right)\right]^2}$$

where e is the vapour pressure in mb, u is in cm sec^{-1}, d is in cm, E is in g cm^{-2} sec^{-1} and T is the absolute temperature. Here we are neglecting the variation of T over the height interval z_1 to z_2. Values of E were calculated from Eq. (2) written in this form for all the periods of observation, and when two sets of apparatus were exposed the E's were calculated from the results provided by each set. T was always taken as the absolute temperature at the greater height and $(e_1 - e_2)$ was calculated from the usual psychrometric formulae.

Before Eq. (4) can be applied it is necessary to know z_0 and β. z_0, like d, depends on the crop height and wind speed and it would be possible to construct a figure similar to Fig. 3 for z_0, the required values being obtained from the plots of u_z against ln $(z - d)$ in neutral conditions. β may be determined by the use of either of the following expressions

$$\frac{u_3 - u_2}{u_2 - u_1} = \frac{(z_3 - d)^{1-\beta} - (z_2 - d)^{1-\beta}}{(z_2 - d)^{1-\beta} - (z_1 - d)^{1-\beta}} \quad . \quad . \quad . \quad (5)$$

$$\frac{u_2}{u_1} = \frac{(z_2 - d)^{1-\beta} - z_0^{1-\beta}}{(z_1 - d)^{1-\beta} - z_0^{1-\beta}} \quad . \quad . \quad . \quad (6)$$

Confining our attention to Eq. (5) we see that β may be obtained once the value to be assigned to d is known. It is possible to use Fig. 3 as a basis of another figure relating d to the crop height for each wind speed from which, by interpolation, the d corresponding to any combination of crop height and wind speed could be read. It would, however, have to be assumed that the smooth curves drawn for the wind speed groups in Fig. 3 give the correct values of d for the mean wind speed of each group. Once the curves of Fig. 3 are accepted as a true representation of the values of d, there is no difficulty. But we must consider the likely error in d and its influence on the determined values of β. It appears that it is necessary to know d with certainty to at least the nearest centimetre before β can be determined with any confidence. An example will make this clear. On 13 July, 14 to 15 GMT, the mean wind speeds at 145, 110 and 70 cm were 317, 291 and 241 cm sec^{-1} respectively and the plot of these observations together with the wind speeds at 245, 195 and 95 cm indicated that the wind speed at 200 cm was 350 cm sec^{-1}. The value of d taken from the smooth curve D of Fig. 3 is 28 cm, which, with Eq. (5), leads to a value of β of 1·04. The scatter of the individual determinations of d suggests that its value might have been as low as 26 cm or as high as 30 cm and these values of d lead to values of β of 1·07 and 1·01 respectively.*

An uncertainty in β of this order results in turn in a large range in the values of E determined from Eq. (4) even when the variation in z_0 is neglected. Thus, without a detailed knowledge of the variation of d with crop height, a knowledge which the present observations do not in general provide since the crop height was changing from day to day and insufficient determinations were obtained on any one day, we are unable to use Eq. (4) with any confidence.

The reliability of the observations of the mean wind speeds also deserves attention in this respect. Consider the example given above. If d is fixed at 28 cm, β has the value

* This range of possible values of d also produces uncertainties in the values of E calculated from Eq. (2). For the example given, the possible error is 5 per cent.

1·04 as already stated. The anemometers used probably do not provide a measure of the wind speed to an accuracy greater than 1 per cent and if we allow the wind speeds given to vary by this amount, the resultant variation in β is alarming. The effect is greatest for a variation in the wind speed at the intermediate height and if we permit the indicated wind speed at this level to increase and decrease by $\frac{1}{2}$ per cent (the wind speeds at the other two heights remaining as stated) the value of β changes from 1·04 to approximately 1·15 and 0·9 respectively. A number of hourly observations was examined and some were found to lead to impossible values of β, which is now not surprising. Thus, while it is theoretically possible to specify the rate of evaporation in unstable conditions by the use of Eq. (4), the uncertainties in the observations do not generally permit this to be done.

There was one period of observation, the last, after the crop had been reaped, when the height of the surface covering was constant. Moreover such values of d as were obtained in this period showed no systematic variation with wind speed. This may have been partly due to the wide limits within which d could be determined, but at least any variation of d with wind speed was small compared with the heights of measurement. The application of Eq. (4) has therefore been restricted to the observations made in this period. In order to obtain values of β which did not depend upon the accuracy of a single observation of the wind profile, Eq. (6) was used in the following way. Two of the heights of wind-speed measurement were 160 cm and 60 cm, and the ratio u_{160}/u_{60} was plotted against the Richardson Number at 40 cm for each hour. The height, 40 cm, was chosen as it is the approximate logarithmic mean of 60 cm and 25 cm, two heights for which the difference in temperature was measured. An approximate Richardson Number was calculated from

$$Ri_{40} = \frac{122\,(T_{60} - T_{25})}{(u_{60} - u_{25})^2}$$

which is arrived at by neglecting Γ, the dry-adiabatic lapse rate (-1×10^{-4} °A cm^{-1}), assuming an absolute temperature of 288°A and a logarithmic variation of wind speed and temperature with height. The latter assumption cannot lead to any appreciable error in the gradients of these elements at the mean logarithmic height and it avoids the need to plot the elements against height so that the gradients may be found by tangent drawing. From the plot of u_{160}/u_{60} against Ri_{40}, the value of the ratio corresponding to $Ri_{40} = 0$ was read. This was 1·34 and by substituting this, together with $d = 6$ cm (see Fig. 3), in Eq. (1), a value of $z_0 = 2{\cdot}47$ cm was obtained. Using this value for z_0 in Eq. (6) a curve relating Ri_{40} to β, was obtained. This curve was then used to read off the value of β appropriate to each hour of observation in period 8 and Eq. (4) used to calculate values of E for the hours during which the atmosphere was unstable in the layer concerned. The equation was not applied directly to the observations of $(e_1 - e_2)$ and $(u_2 - u_1)$, but the following process was employed. Writing the E of Eq. (2) as E_n and that of Eq. (4) as E_u we have

$$\frac{E_n}{E_u} = \frac{[(z_2 - d)^{1-\beta} - (z_1 - d)^{1-\beta}]^2}{(1-\beta)^2\, z_0^{2(1-\beta)} \left[\ln\left(\frac{z_2 - d}{z_1 - d}\right)\right]^2}$$

Values of this ratio for z_1 and z_2 appropriate to both sets of apparatus exposed during the period were calculated for a range of value of β. The hourly values of E_n had already been calculated and in order to obtain E_u these were divided by the ratio E_n/E_u corresponding to the β for the particular hour.

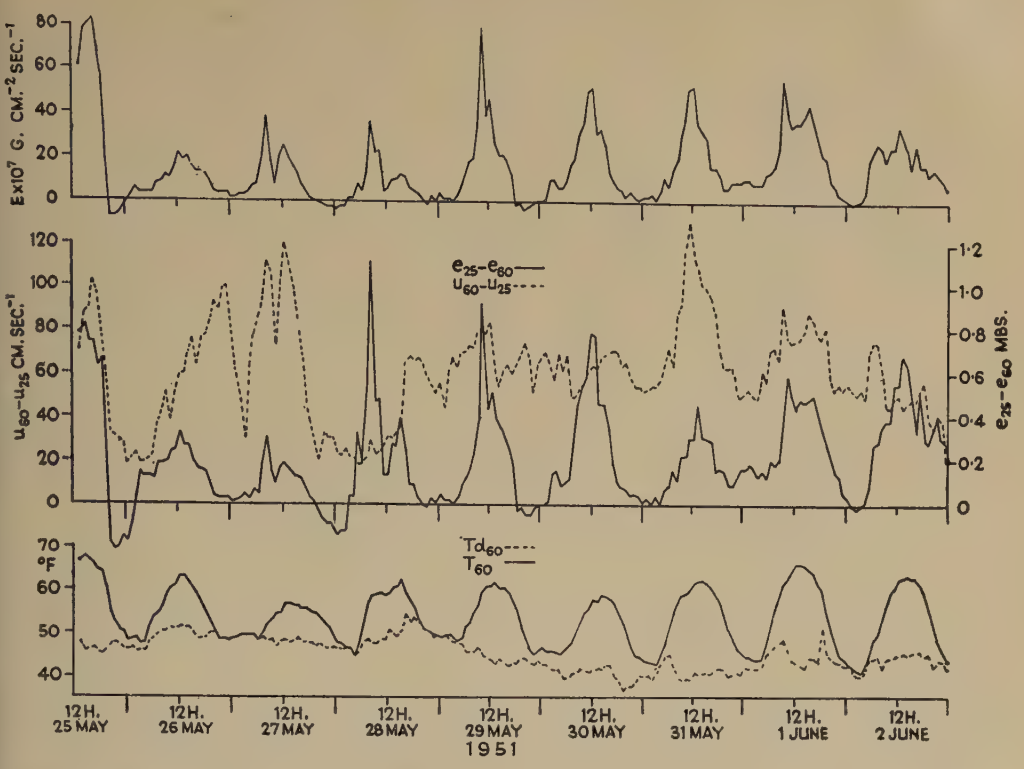

Figure 4. Hourly values of the rate of evaporation (E), vapour pressure difference ($e_{25} - e_{60}$) and wind-speed difference ($u_{60} - u_{25}$) over the height interval 25 to 60 cm, and temperature (T_{60}) and dew-point temperature (T_{d60}) at 60 cm in part of observational period 1.

5. Discussion of the results

The hourly rates of evaporation calculated by means of Eq. (2) for the first 9 days of observational period 1 are shown in Fig. 4. Curves of $(e_{25} - e_{60})$, $(u_{60} - u_{25})$, T_{60} and the dew point at 60 cm, T_{d60}, are also shown. The normal diurnal range of temperature is well illustrated together with the much smaller range of the dew-point temperature. The values of $(u_{60} - u_{25})$ are generally greatest during daylight and this feature, together with the marked variation of $(e_{25} - e_{60})$ from day to night leads to the high values of E during the day and the low values at night. When $(e_{25} - e_{60})$ is negative, it follows from Eq. (2) that E must be negative, indicating a downward transport of water vapour to the crop and ground.

During observational period 7, two sets of apparatus were exposed and the hourly values of E calculated, using Eq. (2), from the results provided by both sets for part of this period, are shown in Fig. 5. The same diurnal range of E is apparent but the main point to note from these curves is that during the day when the rate of evaporation is high the values of E calculated from the results provided by apparatus set B (heights of measurement 210 and 110 cm) are lower than those calculated from the results provided by set A (heights of measurement 125 and 85 cm). This is in accordance with Pasquill's (1949a) result, namely that the use of Eq. (2) in unstable conditions must lead to an underestimation of E, the magnitude of the underestimation depending on the degree of instability as indicated by the Richardson Number. Now the numerical value of the Richardson Number increases with height in the layer of air near the ground and the underestimation

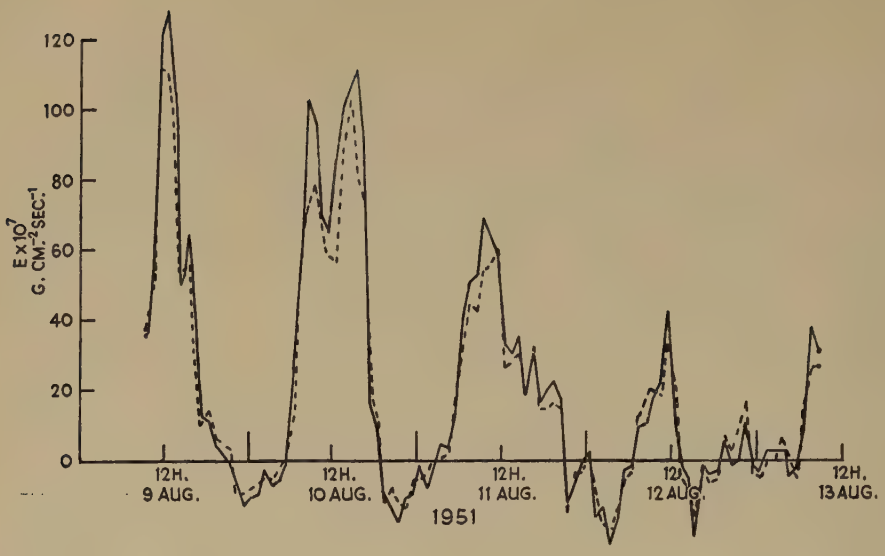

Figure 5. Hourly rates of evaporation during part of observational period 7, computed from the results provided by apparatus set A (————) and set B (– – – –), using Eq. (2).

in E should therefore increase on any unstable occasion as the heights of measurement of the wind speeds and humidities increase. In all the observations there is a casual error in addition to the systematic error arising from the neglect of the stability effect and in the stable case when the rates of evaporation were low, the casual error has hidden the effect of the systematic error.

Fig. 6 shows a plot of E from the results provided by apparatus sets A and B on 23 and 24 August, two days in period 8. In the hours in which the layer of air concerned was unstable the values of E calculated by means of Eq. (4) have been added. Here again we see that the lower values of E are obtained from the results provided by apparatus set B when Eq. (2) is used in unstable conditions. For both days the agreement between the values of E provided by set A and set B is improved by the use of Eq. (4). The underestimation to be expected from the use of Eq. (4) for any one value of β decreases as the heights of measurement decrease. For example, if $\beta = 1{\cdot}03$ and z_1 and z_2 are 85 and 125 cm respectively, $E_n/E_u = 0{\cdot}805$ whereas when z_1 and z_2 are 25 and 60 cm, $E_n/E_u = 0{\cdot}855$. The mean heights of measurement of the humidities and wind speeds and not the separation of the levels is the main factor controlling the magnitude of this ratio.

Since the majority of the hourly rates of evaporation have been calculated by the use of Eq. (2), it is necessary to assess the errors likely to be present. The total error for any one hour will be partly composed of a casual error arising from instrumental imperfection and partly due to the systematic error introduced by the neglect of the stability influence. The casual error, taking into account the possible errors in the assumed values of d, is thought to be of the order of 10 per cent except during the hours when the rate of evaporation is small. An estimate of the systematic error in unstable conditions based on the results for period 8 suggests that on a few occasions this was as large as 20 per cent. No estimate of the magnitude of this systematic error in stable conditions can be made from the present results but we might expect the error to be of the same order.

The daily totals of evaporation are given in Table 2 and it is desirable to know the likely error in these results. To some extent the underestimation during the day will be

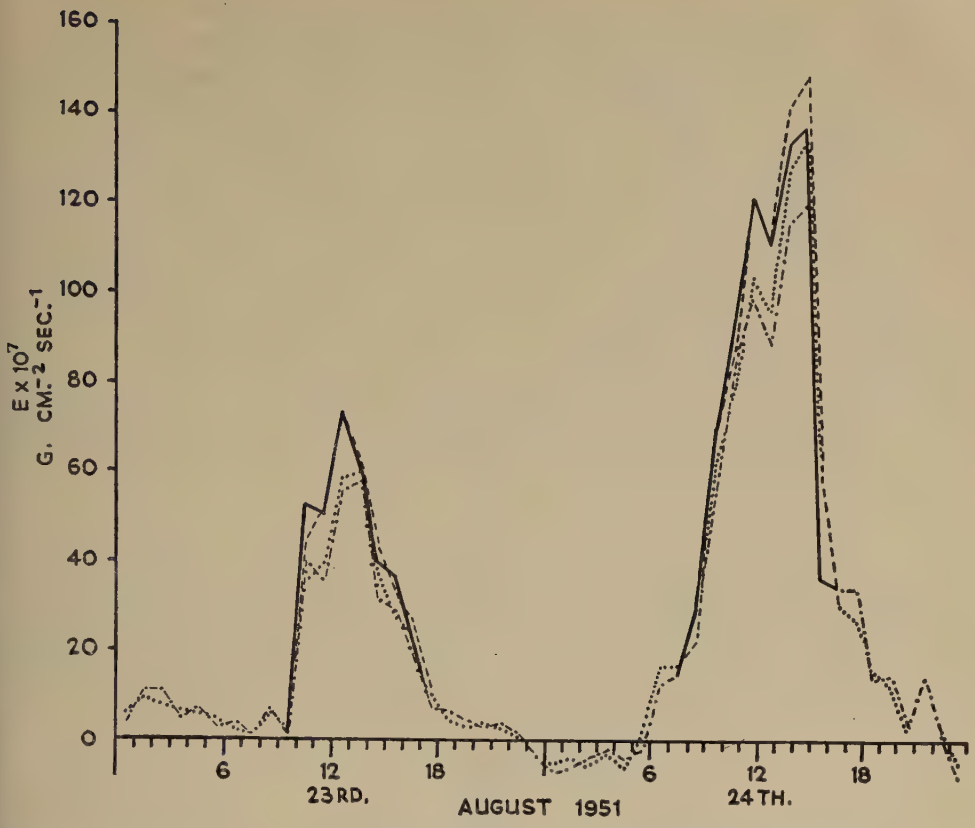

Figure 6. Hourly rates of evaporation computed from observations with apparatus set A and Eq. (2) (. . . . .), set A and Eq. (4) (– – – –), set B and Eq. (2) (– · – · – · –) and set B and Eq. (4) (———).

balanced by an overestimation at night but since the rates of evaporation at night are small compared with those in the day, the totals will all be underestimates. Since the nocturnal evaporation makes such a small contribution to the daily totals, we may neglect its overestimation as a first approximation and assume that the daily totals for the three complete days of period 8 for which Eq. (4) was used in the unstable hours are satisfactory. The mean difference between the daily totals for the three complete days from the results provided by the two sets of apparatus when Eq. (4) is used in the unstable conditions is about 1 per cent. For set A the mean underestimation arising from the use of Eq. (2) for all hours is 9 per cent and the corresponding figure for set B is 16 per cent. An examination of the approximate values of the Richardson Number throughout the whole series of observations suggests that the underestimation in period 8 is typical. Although, when the crop reached its maximum height (period 7) it was necessary to expose the set A apparatus at heights equal to those used for set B in group 8, the error was not so large as the effective height of exposure was less, the much larger values of d during this period having to be taken into account. It is considered that the daily totals of evaporation calculated from the results of apparatus set A by the use of Eq. (2) are underestimates to an extent not exceeding 15 per cent. On many days the error is less.

During the periods of rain and showers, the accuracy of measurement of the wind speeds and dry- and wet-bulb temperatures was doubtful and the daily totals for days with rainfall should be treated with caution.

TABLE 2. COMPUTED EVAPORATION (mm OF WATER)

Date (1951)	Time (if other than 24 hr)	Evaporation by set A and Eq. (2)
Period 1		
25 May	13-24	1·48
26		0·70
27		0·74
28		0·52
29		1·28
30		1·38
31		1·52
1 June		1·92
2		1·40
3		1·18
4		1·01
5		1·55
6	00-10	0·50
Period 2		
11 June	10-24	2·38
12		3·98
13		3·62
14		2·49
15		2·03
16	00-10	0·36
Period 3		
25 June	11-24	2·09
26	00-10	1·14
Period 4		
2 July	15-24	2·28
3		5·83
4	00-10	2·73
5	16-24	1·09
6		5·06
7		3·50
8		3·40
9	00-12	3·12
Period 5		
9 July	12-24	2·53
10		4·87
11		2·92
12		4·28
13		3·22
14	00-10	0·11

Date (1951)	Time (if other than 24 hr)	Evaporation by set A and Eq. (2)
Period 6		
19 July	10-24	4·46
20		3·79
21		1·95
22		1·22
23		0·05
24		1·31
25		2·53
26	00-11	0·43

Date (1951)	Time (if other than 24 hr)	Evaporation by set A and Eq. (2)	Evaporation by set B and Eq. (2)
Period 7			
9 Aug.	09-24	2·17	2·03
10		3·06	2·43
11		1·91	1·69
12		0·04	0·17
13	00-09	0·25	0·22
14	16-24	0·15	0·17
15		2·60	2·31
16		1·41	1·23
17	00-09	0·09	0·10
Period 8			
20 Aug.	14-24	1·82	1·81
21		5·48	5·25
22	00-09	0·69	0·69
23	16-24	1·30	1·26
24		2·90	2·70
25	00-10	0·22	0·20

Date (1951)	Time (if other than 24 hr)	Evaporation by set A and Eq. (4)	Evaporation by set B and Eq. (4)
Period 8			
20 Aug.	14-24	2·09	2·15
21		5·97	5·97
22	00-09	0·70	0·70
23	16-24	1·49	1·51
24		3·18	3·06
25	00-10	0·20	0·23

6. CONCLUSION

The present work suggests that the daily evaporation from cropped surfaces may be obtained to within 15 per cent when the simple aerodynamic formula is used to determine the hourly rates of evaporation from observations provided by apparatus suitably exposed. Previous investigations (e.g., Pasquill 1949a) have shown that an improvement in the accuracy of the results can be expected from the use of the modified form of the simple formula for such hours of the day as have unstable conditions of flow, but unfortunately the difficulties which arise when an attempt is made to apply this modified formula make it of limited practical value, and it is doubtful if it will ever be of use in those cases where the surface-roughness characteristics are changing from day to day with wind speed. Apart from the difficulties of specifying the roughness characteristics of any surface, the accuracy with which it is possible to determine the mean wind speeds with cup

anemometers is generally insufficient to yield reliable estimates of the value of the index in the Deacon wind-profile law. Experience in the field suggests that, even if a better instrument were available, its use would not be justified as the horizontal variation of the mean wind speed appears to be of the same order as the accuracy of the present anemometers. It might be possible to determine the corrections to be applied to the rates of evaporation, determined by the use of the simple formula, by exposing three or more sets of apparatus simultaneously at different levels. Since the errors due to the stability effect decrease as the surface is approached, it should be found that the computed values of E approach a limit as the heights of exposure of the apparatus decrease. The Richardson Number could then be computed and the corrections to be applied to the value of E calculated from the results provided by apparatus exposed at any height could be stated in terms of the Richardson Number. Unfortunately it seems likely that, even if this proved possible, it would be necessary to repeat the procedure for each different surface as it is by no means certain that there is one relationship between β and the Richardson Number which is applicable to all surfaces irrespective of their roughness characteristics.

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Rainfall in relation to the jet stream

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SUMMARY

A study is made of the rainfall occurring at four British stations on occasions when they lie beneath the jet stream. The mean distribution of rainfall in relation to the jet stream provides evidence, in agreement with dynamical theory, for a direct circulation at the jet entrance and an indirect circulation at the jet exit. Precipitation patterns found for uniform currents provide no satisfactory evidence with regard to the preferred vertical circulations within the central region of the jet stream.

1. INTRODUCTION

There is reasonable evidence for the existence of a broad association between some types of rainfall and the jet stream. Chaudhury (1950) and Yeh (1950) have shown that the rainfall maxima in the mean winter precipitation patterns for India and China lie beneath the Asian low-latitude jet stream. The Himalayas complicate the Indian picture, but the Chinese maximum is clearly elongated in the direction of the upper flow. Starrett (1949), by comparing the locations of North American jet streams with 24-hr precipitation amounts, found that over America too precipitation is greater beneath the jet stream than beneath the relatively slow-moving air to the north or south.

That there should be such an association is understandable in view of the recognized relation between middle-latitude jet streams and the polar front, and in view of the capacity of jet streams to act as steering currents for polar front and other cyclonic disturbances. In some of the recent literature, however, it is suggested that a more intimate relation between rainfall and the jet stream may exist, depending on the preferred vertical circulation pattern within the flow. It is the purpose of the present paper to examine this suggestion with the aid of rainfall data from stations in the British Isles.

2. SELECTION OF DATA

Occasions were selected when one or more of the four stations, Lerwick (60°N, 01°W), Tiree (56°N, 07°W), Valentia (52°N, 10°W) and Scilly (50°N, 06°W) were situated beneath a jet stream. Fig. 1 shows the location of each station.

The jet streams were chosen with the aid of 300-mb contour charts prepared as a six-hourly routine at the Central Forecasting Office, Dunstable over the years 1949-51. They were required to be well-defined features of the upper flow with single axes: weak, complex, or local wind maxima were excluded. The upper currents were divided into three regions for the purpose of the analysis, the jet entrance or confluence, the centre and the jet exit or difluence (Fig. 2). Occasions when the flow at the axis of the jet (and also at the station in question in the case of the central region) had appreciable curvature were discarded, i.e., portions of the jet stream which flowed around the crest of an upper ridge or the nadir of an upper trough were not included. No special restriction was placed on the orientation of the jet but since advantage was taken of every suitable occasion, westerly and north-westerly jet streams predominated.

Figure 1. Locations of observing stations.

Figure 2. The sectors of the jet stream

The location, relative to a jet stream, of any of the selected stations at 03 GMT or 15 GMT was obtained from inspection of the 300-mb contour charts and the distance of the station from the 300-mb jet-stream axis was measured. The position of the jet axis was estimated, taking into account continuity between the six-hourly charts. The amount of rainfall attributed to each occasion refers to the 12-hr period centred about the time of the relevant contour chart; it was obtained from the data published in the *Daily Weather Report*.

3. Analysis of data

The rainfall measured at any station depends upon a number of factors, some of which are functions of the synoptic situation and others, such as topography, which may be purely local characteristics. In a statistical study of the relations between rainfall and the synoptic situation variations due to local effects must be eliminated as far as possible. Ideally, we might have used rainfall data from a single observing station, but this proved to be impracticable in the present instance. Although the four selected observing stations are in some respects similarly situated, the mean annual rainfall, as shown by Fig. 1, varies appreciably from one to the other. This variation is due partly to local effects and partly to the locations of the stations relative to certain features of the mean atmospheric flow patterns. Local effects, however, appear to predominate: for example, the distance between Scilly and Valentia is negligible on the scale of the mean circulation, yet these stations have the least and greatest average rainfalls. In order to test whether the statistics relating to either side of the jet axis are unduly biased by the inclusion of too many data from a relatively wet or dry station, an index of bias has been formed. The index is the

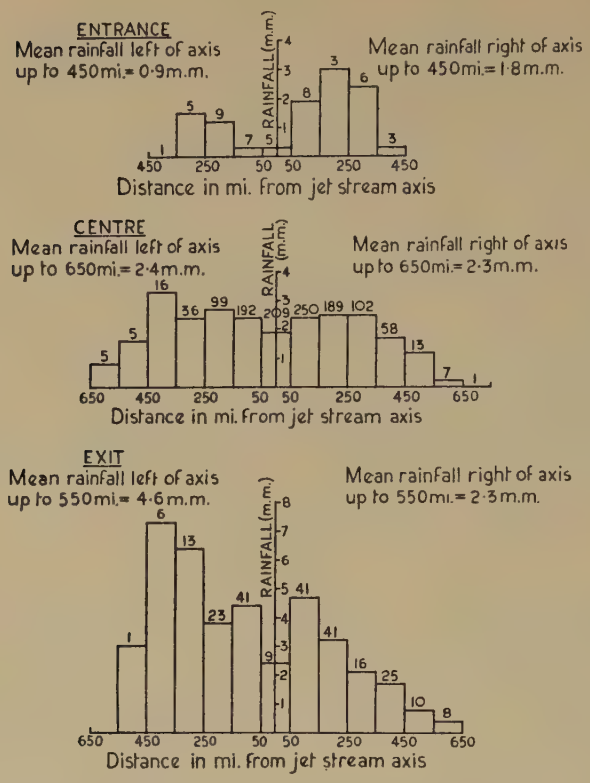
ENTRANCE
Mean rainfall left of axis up to 450mi.= 0·9m.m.
Mean rainfall right of axis up to 450mi.= 1·8m.m.
RAINFALL (m.m.)
Distance in mi. from jet stream axis
CENTRE
Mean rainfall left of axis up to 650mi.= 2·4m.m.
Mean rainfall right of axis up to 650mi.= 2·3m.m.
RAINFALL (m.m.)
Distance in mi. from jet stream axis
EXIT
Mean rainfall left of axis up to 550mi.= 4·6m.m.
Mean rainfall right of axis up to 550mi.= 2·3m.m.
RAINFALL (m.m.)
Distance in mi. from jet stream axis

Figure 3. Distribution of rainfall beneath the jet stream. Figures above the columns give the number of cases for each interval.

weighted mean defined by the formula $I = \sum_i A_i r_i / \sum_i r_i$ where A_i is the mean annual rainfall for a given station, and r_i, the number of observations from that station included in the sample under test, the summation being taken over the four stations.

Fig. 3 shows the average rainfall distribution relative to the jet-stream axis for cases of confluence, uniform flow and difluence. The distance of a station from the jet axis was measured for each occasion to the nearest 10 mi, but the position of the axis itself was not known to that degree of accuracy and the data have been combined to give means over 100-mi steps.

Fig. 4 is a composite frequency diagram giving the percentage frequency of occurrence of large amounts of precipitation (more than 10 mm in 12 hr), moderate amounts (3 to 9·9 mm in 12 hr), small amounts (trace to 2·9 mm in 12 hr) and no precipitation.

TABLE 1. INDICES (IN CM) OF BIAS DUE TO LOCAL EFFECTS

	Left entrance	Right entrance	Left centre	Right centre	Left exit	Right exit
Overall distribution (Figs. 3 and 4)	10·9	11·5	10·5	10·9	10·3	10·6
Rain only (Figs. 5 and 6)	—	—	10·4	11·1	10·0	10·5
Showers only (Figs. 5 and 6)	—	—	11·0	11·3	11·1	11·1
Both rain and showers (Figs. 5 and 6)	—	—	10·8	11·1	10·4	11·0

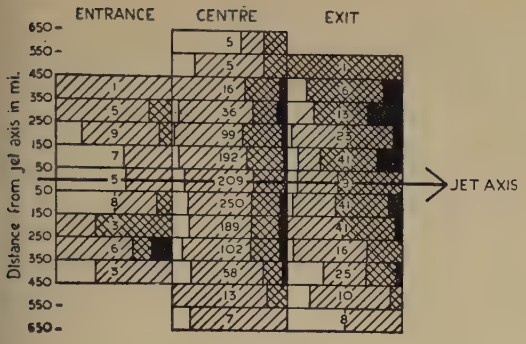

Figure 4. Composite frequency diagram of precipitation in the jet stream. The shaded area is proportional to the percentage frequency. The enclosed figures give the number of cases in each division.

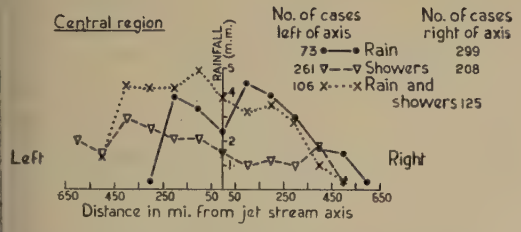
Central region
No. of cases left of axis
No. of cases right of axis
73 Rain 299
261 Showers 208
106 Rain and showers 125
RAINFALL (m.m.)
Left
Right
650 450 250 50 50 250 450 650
Distance in mi. from jet stream axis

Figure 5. Variation of mean rainfall distribution with precipitation type in the central region.

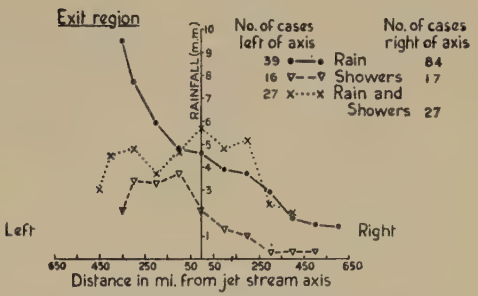
Exit region
No. of cases left of axis
No. of cases right of axis
39 Rain 84
16 Showers 17
27 Rain and Showers 27
RAINFALL (m.m.)
Left
Right
650 450 250 50 50 250 450 650
Distance in mi. from jet stream axis

Figure 6. Variation of mean rainfall distribution with precipitation type in the exit region.

The indices of bias due to local effects contained in the data summarized in Figs. 3 and 4 are given in the first row of Table 1. From these indices we conclude that the selected occasions are so distributed amongst the four stations as to introduce a small bias of about five per cent in our estimate of the average precipitation falling to the right of the axis as compared with precipitation falling to the left.

The data for the central and exit regions (but not for the entrance) were sufficient to permit further classification according to the character of the precipitation. The nature of the precipitation was determined from the descriptions of the weather, in terms of Beaufort letters for the appropriate twelve-hour periods, contained in the *Daily Weather Report*. Three classes were formed : (a) when only showers were reported, (b) when both rain and showers were reported for the same 12-hr interval and (c) when rain alone was reported. The distributions of rainfall for each class in the two regions are shown in Figs. 5 and 6. Since the number of cases comprising each class in the exit is small, the distributions for this region have been smoothed by taking mean values for overlapping 300 mi intervals. The indices given in rows two to four of Table 1 show that there is very little bias due to local effects.

4. Discussion

Figs. 3 and 4 indicate that precipitation is distributed relative to the jet stream according to a fairly definite pattern. In particular :

(i) in the entrance, twice as much rain occurred to the right of the axis as to the left;

(ii) in the central region the amounts to the right and left of the axis were practically equal;

(iii) in the exit, there was twice as much rain to the left as to the right of the axis.

This observed rainfall pattern is the consequence not only of the mean vertical circulations but also of the average distributions of other factors such as the degree of convective instability and the quantity of water available for precipitation. Near the jet stream, the mean temperature of the troposphere normally decreases towards the left of the flow much more quickly than the temperature of the underlying surface. Consequently, the chance that showers will be set off by surface warmth and the frequency and intensity of such showers will increase towards the left of the flow. Showers, it is true, may be intensified by larger-scale convergence, but nevertheless, the effects of broad-scale upward motion will be shown most clearly in precipitation reported as rain. The cross-jet distribution of humidity is not known for the occasions used in this study. Experience with vertical cross-sections suggests, however, that the humidity mixing ratio (but not necessarily the relative humidity) will be greater on the average to the right of the jet axis than to the left. This is confirmed for the middle and upper troposphere by the mean humidity distribution prepared by Murray (1953) on the basis of soundings made within jet streams by aircraft carrying the Dobson-Brewer frost-point hygrometer. It follows that, once saturation is attained, a given vertical lifting will result in the precipitation of a greater amount of moisture to the right than to the left of the axis. The exact magnitude of the effect cannot be calculated from the available data. Nevertheless we can estimate that under average conditions the effect could result in there being up to fifty per cent more rain precipitated to the south of the axis than to the north for the same vertical motion. It is hardly possible, however, that this effect by itself could account for the observed distribution of precipitation in the jet entrance, where twice as much rain fell in the region up to 450 mi south of the axis as in the region up to 450 mi north of the axis.

Examination of Fig. 5 shows that in the central region, the distributions of amounts due to rain and amounts due to showers are very different. Shower-type precipitation increases towards the left of the flow but amounts due to rain-type precipitation tend to be greater to the right of the axis than to the left. It is evidently unsafe to deduce from the symmetric distribution of precipitation amount given in Fig. 3 for the central region, that there is no preferred large-scale vertical motion pattern. On the other hand it is doubtful whether the asymmetry about the jet axis of the distribution for the rain-type cases given in Fig. 5 is sufficiently well marked for us to be certain that it cannot be accounted for in terms of the cross-flow gradient of humidity mixing ratio. We consider in fact, that the data are inadequate to decide whether there is any preferred vertical circulation within the central region of the jet stream.

From Fig. 6 it is seen that in the difluence, rain-type precipitation increases markedly towards the left of the flow. The elimination of shower-type precipitation does not affect the broad pattern for this sector and the preponderance of rainfall to the left of the axis appears genuinely to be due to large-scale dynamical effects. It is also interesting to note from comparison of Figs. 5 and 6 that shower-type precipitation amounts increase more sharply from right to left in the exit than in the centre.

5. Implied vertical motions

The preponderance of rainfall to the right of the confluence and to the left of the difluence is indicative of mean vertical circulations within the current in the sense required by the flow accelerations and associated developments according to Sutcliffe's theory (Sutcliffe 1938, 1947). More explicitly: the acceleration of the flow at the confluence indicates that an ageostrophic flow takes place down the pressure gradient, that is from right to left across the jet axis in the upper troposphere, with convergence resulting to the left and divergence to the right of the axis. A compensating return flow, with divergence

to the left and convergence to the right occurs in the lower troposphere. To complete the cross-axis circulation model, upward vertical motion is required to the right of the axis and downward vertical motion to the left. This type of vertical circulation, occurring in the direction of natural convective overturning, has been referred to as a 'direct' circulation. A cross-axis circulation in the opposite ('indirect') sense should occur at the difluence. These effects have been discussed in detail by several writers. Murray and Daniels (1953) in particular have demonstrated statistically the existence of the appropriate cross-axis flows at jet stream levels. The present study affords evidence for the associated vertical motions.

A comparison between 24-hr precipitation patterns near jet streams and the location of the jet axis at the end of the precipitation period has been made by Starrett (1949). Starrett concluded that the amount of precipitation (expressed as a percentage of the monthly normal) to the north of the axis exceeds that to the south, and claimed that this study supports the vertical motion pattern required by the theory of jet-stream formation proposed by the Staff Members, University of Chicago (1947). Two types of jet streams were considered: 'jet streams with troughs' and 'quasi-uniform jet streams.' In the trough cases a pronounced maximum frequency of rainfall occurred north of the axis and east of the trough line. However, Starrett's Fig. 1 indicates that on the average, the air was decelerating as it moved downstream from the axis of the trough. Thus, the situation of the rainfall-frequency maximum is readily accounted for in terms of the flow accelerations, and it is not necessary to invoke the Chicago theory to account for the implied vertical motion pattern. In the quasi-uniform cases, the highest frequency of precipitation occurs close to the jet axis and there is only a slight tendency for frequencies north of the axis to be greater than those which occur to the south. Starrett has not considered the possible effects of convective instability or of moisture content, and in addition, it is clear that residual geographic effects are contained in the frequency patterns. In view of our experience in the present work we doubt whether Starrett's precipitation patterns can legitimately be held to give evidence for an indirect circulation in the case of uniform flow.

6. Conclusions

The mean rainfall patterns at the entrance and exit of the jet stream imply mean cross-axis vertical circulations with ascent beneath the right entrance and left exit, and with descent beneath the left entrance and right exit. These circulations agree with accepted dynamical theory. No definite conclusions with regard to preferred vertical circulations within the central region of the jet stream can be made from arguments based purely on the mean precipitation patterns found for uniform currents.

Acknowledgments

We wish to thank Mr. J. S. Sawyer for his helpful advice. This paper is published by permission of the Director, Meteorological Office.

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Note on the structure of the high altitude strong-wind belt in the Middle East in winter

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(Manuscript received 27 November 1953)

Summary

By referring the upper-air observations at Habbaniya and Bahrein for January 1951 to the estimated central axis of the strong stream a meridional cross-section is constructed showing the mean structure for that month of the strong-wind belt in the upper troposphere over Iraq. This mean structure should apply to the stream on a particular day in contrast to the mean flow as usually computed which shows the average conditions above each observing station. The stream is found to be broad and without strong horizontal gradients. Its axis is at about 190 mb, its mean position about 32°N and the breadth of the region of highest speeds (100 kt or more) about 800 mi.

1. Introduction

The strong winds which blow in the upper troposphere over the Middle East in winter and spring are well known. Frost (1953) and Gilchrist (1953) have shown meridional cross-sections of the mean flow of the air over Iraq in January and April. This strong-wind belt can certainly be associated with the polar front. It is also probably in part a continuation of a strong flow which is found in the upper troposphere in many longitudes between lats. 25°N and 40°N (Gilchrist 1953, Namias and Clapp 1949). The structure of this strong-wind belt, whether it resembles a jet stream of more temperate latitudes or is less sharply defined, is in some doubt, as the averaging process employed in depicting the mean flow over a region for a particular period, say one month, may well obscure interesting features of the flow.

The following note describes an attempt to discover the structure of this stream from a study of the observations at Habbaniya (33°22′N, 43°34′E) and Bahrein (26°16′N, 50°37′E) twice daily in Jan. 1951. The method of analysis was to take the axis of the strong stream as a reference and to group the observations at the two stations according to their distance from this axis. When the mean of the grouped observations is taken over the whole month the results should typify the stream on some particular occasion. In contrast, since the axis of the stream varies in position from day to day, the mean flow obtained in the ordinary way by averaging a month's observations, station by station (what may be called the geographical mean flow), cannot be expected to resemble the flow on any particular day.

2. Method of analysis

Temperature and wind data for Habbaniya and Bahrein for the levels 400 mb and upwards were plotted for each hour of observation in January 1951 as shown in the examples in Fig. 1 which give data for 12 to 14 January. Mean wind direction between 300 mb and 100 mb was given below each plot; variation about this mean was usually less than 15° to 20° – wind direction seldom changes much with height at those levels in the Middle East. Tropopause levels were determined using the experimental definitions given in M.O. 555, Pt. 3.

From the observations plotted as in Fig. 1 an estimate was made of the plan position of the axis of strongest winds relative to the two observing stations. Fig. 2 shows the method of classifying the position of the axis. The positions are recorded on Fig. 1 for the relevant observations. In making this classification consideration was given to continuity and to the rise or fall of temperature in the middle troposphere as indicating northward or southward motion respectively. The estimates are, therefore, subjective and must be liable to considerable error on occasions. It should be mentioned that the analyst may well have been influenced by a knowledge of the typical variations in jet streams of temperate latitudes, but since some of the results of the analysis are dissimilar to those for such a jet stream this source of error may not be serious.

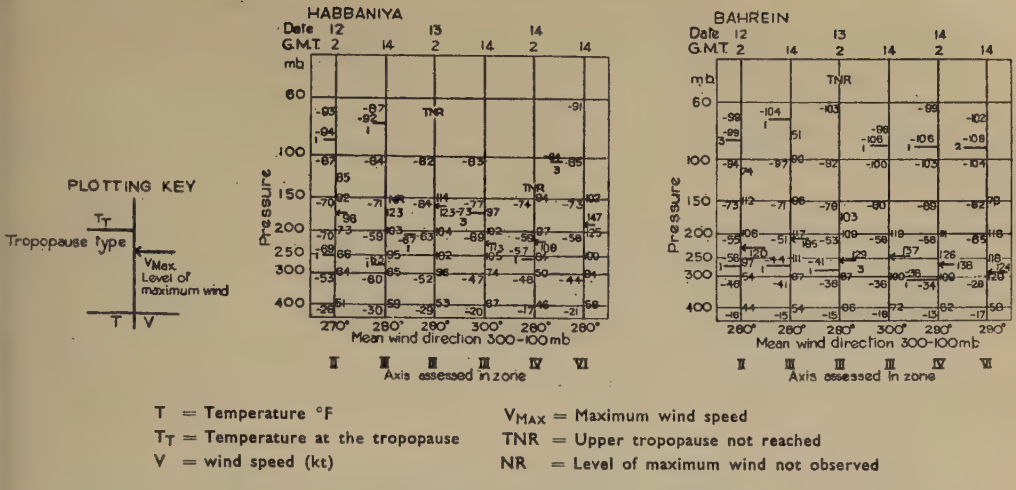

Figure 1. Upper winds and temperatures at Habbaniya and Bahrein, 12 to 14 Jan. 1951.

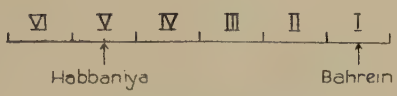

Figure 2. Position of zones relative to Bahrein and Habbaniya.

From each pair of observations approximately 640 mi apart an estimate was made of the width of that part of the stream with a speed of 100 kt or more. Such an estimate can be merely a lower bound to the true width and is probably much too small on many occasions. For example (see Fig. 1) the minimum width of the strong winds at 14 GMT 14 Jan. was estimated as 400 mi, allowing for wind direction and the fact that the wind was appreciably greater than 100 kt at both Bahrein and Habbaniya. These estimates of width were largest when the axis of the stream was north of Habbaniya (zone VI), the wind then often exceeding 100 kt at both stations. This result is to be expected if the axis of the strong winds is nearer the polar edge of the stream. The mean value of such minimum widths was 520 mi for the 21 cases in zone VI.

Next, mean values of the various parameters were evaluated for Habbaniya for occasions with the axis in each of the zones I to VI respectively. The same was done for Bahrein. On many occasions a level of maximum wind was not observed as wind was still increasing with height when observations ceased as the radar target passed out of range. Two computations were therefore made on maximum winds, one a straightforward mean height and speed of all available observations and the second a minimum mean height and speed of the maximum wind obtained by using the last observation of those ascents when a true maximum was not reached.

These mean values for each zone were then plotted as shown in Fig. 3, the horizontal spacing of the observations being a rough measure of the distance of the observations from the axis of the stream. For example, the observations labelled Habbaniya zone IV are the mean of all values observed at Habbaniya when the axis of the stream was estimated to be in zone IV. When the axis of the stream was in zones V or VI, observations at Habbaniya were in the same position relative to the axis as observations made at Bahrein when the axis was in zones I or II respectively. Otherwise there is no overlap in the observations from the two stations. Bahrein observations for zones I and II are plotted separately in Fig. 3. If we assume that northward or southward fluctuations of the position of the stream do not affect its mean characteristics (an assumption which is probably not fully justified; see below) then we may consider the mean values plotted in Fig. 3 as referring to stations at fixed intervals across a mean westerly stream. Habbaniya is 490 mi north of Bahrein and the mean wind in the upper troposphere over Iraq was approximately westerly in January 1951, so that the interval between the plots may be taken as about 120 mi.

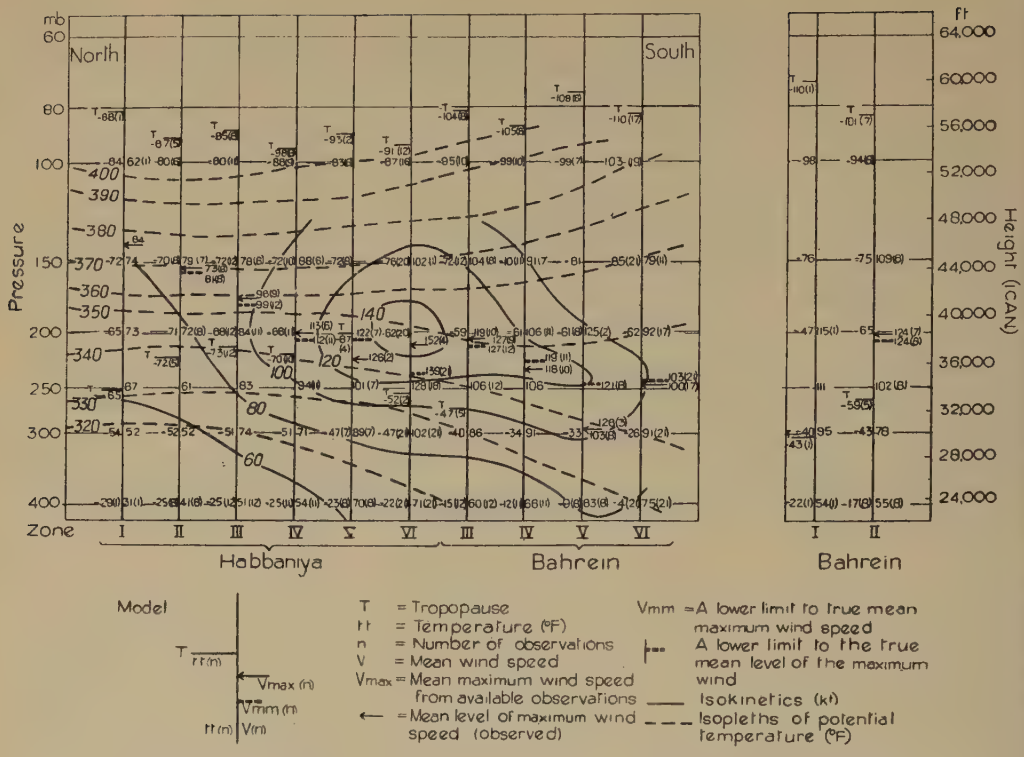

Figure 3. Cross-section of the mean westerly flow in the upper air at 45°E, Jan. 1951.

Isopleths of mean wind speeds have been drawn on Fig. 3. Loss of observations at the higher levels makes some of the plotted mean values at 200 mb and above unrepresentative and some allowance has been made for this in drawing the isokinetics. The minimum heights and speeds of the mean maximum wind computed as described above were also used in assessing the position of the central isopleths.

Isopleths of potential temperature were also drawn on Fig. 3 from the mean temperatures plotted. Like the isokinetics they appear to follow a pattern and, using the geostrophic relation, the slopes of these lines were used to reassess levels of maximum wind and hence to make minor corrections to the isopleths of mean wind.

3. Discussion

It is perhaps surprising that the subjective analysis described in Section 2 should produce such a self-consistent picture as Fig. 3. This consistency probably indicates that on the average the assessments of position of the axis of the stream were not far out and that the assumptions made in the analysis were mainly valid. It will be noted, however, that there is only limited agreement between the observations at Habbaniya zones V and VI and Bahrein zones I and II where the two sets should overlap in Fig. 3. This is some indication that the stream differs in detail (temperatures, tropopause levels, etc.) according as it is north or south of its mean position, though it does not necessarily show that the structure of the stream varies with position.

Another discrepancy in the analysis is that though each pair of observations was classed as in Fig. 2 the axis of the stream in Fig. 3 appears not at Habbaniya V but nearer Habbaniya VI and Bahrein II. This fault is not considered serious, in view of the roughness of the classification.

Points of interest are :

(i) The stream appears to be wide, and horizontal gradients of wind speed are less than in typical jet streams of higher latitudes. The mean width of the strong stream (100 kt or

more) is 800 mi, more than 10° of latitude. That this estimate is not greatly in excess of the true value is shown by the mean lower bound of 520 mi for occasions in zone VI (see section 2). The (geographical) mean flow for Jan. 1951 (Gilchrist 1953) shows the width of the strong stream (100 kt or more) to be just less than 400 mi. Mr. J. S. Sawyer has pointed out in conversation that horizontal gradients of wind speed would be expected to be directly related to the local value of the Coriolis parameter, a condition for dynamic instability being that anticyclonic shear in isentropic surfaces should exceed the Coriolis parameter (e.g., Sawyer 1949). Johnson (1953) quotes mean maximum shears of 40 kt/100 mi (cold side) and 25 kt/100 mi (warm side) for jet streams over the north-east Atlantic in Jan. 1950, the mean latitude of these streams being about 55°N. The greatest horizontal shears in Fig. 3 are about 18 kt/100 mi and 16 kt/100 mi on the cold and warm sides respectively and it is seen that the ratios of these figures to the corresponding figures of Johnson (0·45 and 0·56 respectively) do not differ greatly from the ratio of the respective Coriolis parameters (0·61).

(ii) Assuming zone II to be 120 mi, zone III to be 240 mi . . . . zone VI to be 600 mi north of Bahrein, the mean position of the axis of strongest winds was just less than 400 mi north of Bahrein in Jan. 1951, i.e., about lat. 32°N. This estimate may be a little south of the true position as cases in zone VI, i.e., north of Habbaniya, may have a mean distance from Bahrein of more than 600 mi. This mean position of the axis of the stream is about 2° north of the position shown by Gilchrist (1953) for the axis of the (geographical) mean stream. Such a discrepancy is to be expected since (see Fig. 3) the horizontal gradient of wind is greater to the north than to the south of the axis and, as Davis (1951) has pointed out, this would result in the axis of the geographical mean stream being further south than the mean position of the individual axes.

(iii) The level of maximum wind falls southwards through the strong-wind belt. This is the reverse of what is found in the typical frontal jet stream of middle latitudes. It may be argued that this result may be incorrect, because more ascents failed to reach the level of maximum wind in the stronger winds on the right-hand side of Fig. 3. This effect must certainly be present, but some allowance has been made for this in drawing the isopleths and the weight of the evidence is for the result as stated. For example, there is little doubt that the level of maximum wind is above 200 mb at the centre of the stream and below 200 mb at the point Bahrein IV. This sloping downwards to the south of the level of maximum wind may be detected on the mean meridional section for Jan. 1951 (Gilchrist 1953).

(iv) The upper tropopause does not appear to vary greatly; it is consistently higher at Bahrein than at Habbaniya.

(v) The lower tropopause appears to slope upwards towards the axis of strong winds but the evidence for this on the south side of the axis is slender.

Acknowledgment

This note is published by permission of the Director of the Meteorological Office.

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551.508.74 : 551.574.4

A sensitive recording dew-balance

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Summary

The instrument described gives a continuous record on photographic paper of the change in weight of a naturally exposed grass surface, 330 cm^2 in area. The sensitivity is 2 mg cm^{-2} per cm of record and the time scale is 0·5 hr/cm. Observations suggest that on very calm nights the condensation of vapour reaching the surface by diffusion from the atmosphere is only a small fraction of the total ' dew.' The maximum rate of deposition that has been observed on short grass is 0·03 mm hr^{-1}.

1. Introduction

Previous studies of the turbulent transfer of water-vapour over grassland have been concerned almost exclusively with evaporation conditions. Since the condensation regime is always associated with a stable stratification of the surface air and often with low wind speeds, the vapour transfer is very small and techniques which have been successfully applied to the direct measurement of evaporation (Pasquill 1950) are insufficiently sensitive for the measurement of dew formation.

Attempts have been made to measure dewfall by finding the increase in weight of an artificial surface exposed in an arbitrary manner; but while data obtained in this way may possess some significance for the botanist or agriculturist, the technique cannot be employed in any serious study of the physics of dew formation. The dewfall on a *natural* surface has been measured by Aitken (1885) and more recently by Lehmann and Schanderl (1942) who made periodic weighings of a large pan containing soil covered with turf. Craddock (1951) constructed a continuously-recording balance for the measurement of dewfall on grass or on a plant. The present instrument is a development of the Craddock model with a more satisfactory suspension and possessing the high sensitivity which the accurate assessment of down-fluxes of water vapour demands.

2. Instrument design

To obtain a representative exchange of water vapour at its surface, it was essential that the weighed specimen should be made as nearly as possible indistinguishable from its environment in thermal and surface properties. It was desirable in the first place that a natural core of soil should extend below the turf surface to a depth at least as great as that in which a marked diurnal variation of temperature could be observed. It was further essential that this core should be ' undisturbed,' i.e., not packed artificially into its container, in order to preserve the natural heat capacity, drainage properties, root structure, etc. Accuracy and representativeness favoured as great a surface area as was possible without making the instrument too unwieldy.

It was found convenient to use as an outer container a Paxolin cylinder 11 in. deep and of 8 in. inside diameter within which could be inserted a metal cylinder with a detachable base holding the soil core. Since this unit weighed about 20 kg and since the *maximum* dewfall anticipated was of the order of 10 g (equivalent to 0·3 mm) it was necessary to employ a suspension which was both robust and frictionless. This was provided by a cross-spring pivot system consisting of pairs of beryllium-copper strips clamped at right-angles to two metal blocks as shown in Fig. 1 (see also Elliot and Home Dickson 1951). If one block is fixed, the other rotates about the axis of intersection of the strips and for small displacements the angle of rotation is proportional to the applied moment. The strip dimensions were calculated from their elastic properties and from their required mechanical sensitivity and strength.

To ensure that the formation of dew on the pan did not cause it to sink appreciably below the level of its surroundings, the deflection from equilibrium produced by a 10 g weight was limited to 0·01 radians. By employing the Kelvin double-suspension mirror system it was possible to magnify linearly this small rotation to give a 14 cm deflection of a light spot on a rotating drum of bromide paper. The sensitivity of the instrument and the desired linear response are

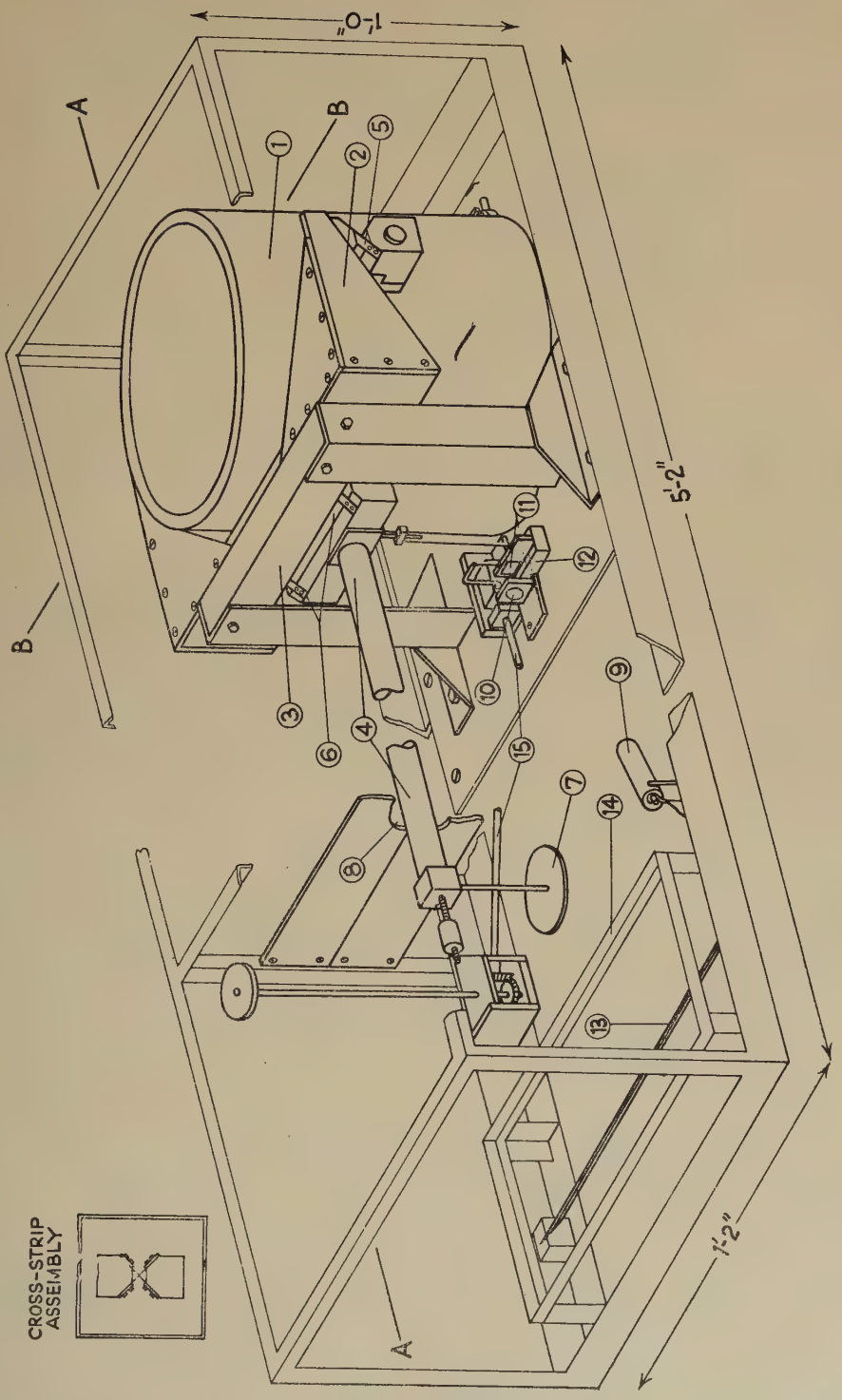

Figure 1. Dissected drawing of dew-balance.

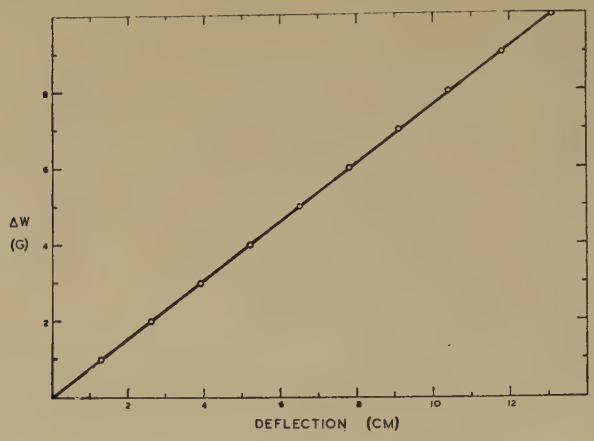

Figure 2. Calibration of the instrument.

illustrated in Fig. 2, obtained by first counterbalancing the soil-pan and then adding weights ΔW. A change in the total weight of the pan of 1 in 10^5 produces a deflection of 2·6 mm and a 1 mm deflection is equivalent to a dewfall of 0·23 mg cm^{-2}.

3. Construction

To obtain the natural soil sample, the metal cylinder with its base removed is stood upright on the grass. The surrounding material is then excavated so that the pan can be pushed downwards into the ground until the turf reaches the top of the pan which then contains an undisturbed core of soil. Free drainage is possible through the bottom of the inner container and water collecting in the bottom of the outer container can be drained off when convenient through a stop-cock.

The mechanical part of the balance consists of four main components, shown in Fig. 1, viz., soil-pan (1), saddle (2), bridge (3), and beam (4). The soil-pan is suspended by two pairs of cross-springs (5) from either side of the saddle to which is bolted the beam, a 3 ft length of 1⅛ in. diameter Dural tube. This assembly is suspended from the bridge by two further pairs of springs (6). A counterweight of approximately 4 kg is required and is carried on the platform (7) at the end of the beam with a small adjustable weight for final balance. The movement of the beam is constrained by a gate (8) and an oil dash-pot (shown in Fig. 3) prevents excessive oscillation.

The optical system consists of the lamp (9) which projects a slit image through the lens (10) to the rotatable mirror (11) which hangs in a dash-pot (12) and is suspended by a 44 s.w.g. wire from the beam at one side and from a fixed support at the other. The light is reflected to the camera section where it is reflected upwards by the mirror (13) into the camera box (shown in Fig. 3) through a 0·001 in. slit. An additional lamp is used for time-marking the record. The camera box contains the recording paper attached to a drum, 48 cm in circumference, driven at the rate of 2 cm/hr by a synchronous motor. It is located in a fixed frame in the base of the balance and is removable for reloading.

With the exception of the camera section, the balance, when sunk beneath the ground, is covered with turf-filled trays (Fig. 3). One of these, with a circular aperture, fits over the Paxolin cylinder with a few millimetres clearance so that the sample presents an almost unbroken surface to the wind, whatever its direction.

4. Operation and practical difficulties

Before each run, the counterweight is adjusted to make the beam horizontal and the suspended mirror is rotated (by a control (15) in the camera section) to bring the light spot to a suitable position. When the camera is in position, the camera section is covered with a draught- and light-tight lid.

During the day, moist surface air is liable to condense on the colder parts of the balance and care must be taken to remove this before operating. At night, however, the balance is warmer than the surface air and there is no danger of condensation occurring except on the surfaces of the pan and trays.

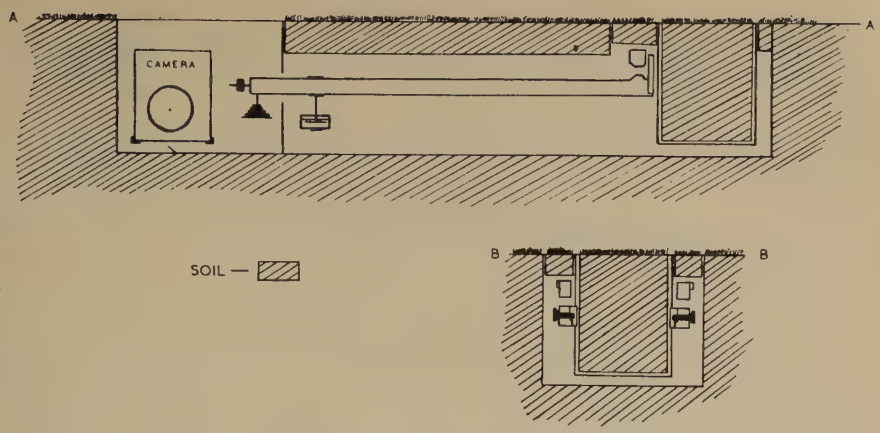

Figure 3. Simplified cross-sections AA and BB (see Fig. 1).

Many of the traces obtained showed violent irregularities which were discovered to have been caused by earth-worms exploring the balance from terra firma. This trouble was eliminated by illuminating the pan throughout the night with a 100-w bulb placed several feet cross-wind of it.

5. Results

During the summer and autumn of 1953, the instrument was in operation in a playing field. Observations were restricted to occasions when the wind direction gave a fetch of at least 200 m over extremely level grass, 1 to 2 cm in height.

(a) *Evaporation*

Two typical traces are shown on Fig. 4 (a) obtained on an afternoon when 6/8 cumulus made sunshine intermittent. The reappearance of the sun from behind a cloud (×) increased the evaporation rate from 0·4 to 2·1 × 10^{-5} g cm^{-2} sec^{-1}. The greatest *condensation* rate yet observed is only 0·08 × 10^{-5} g cm^{-2} sec^{-1}, equivalent to 0·03 mm hr^{-1}.

(b) *Condensation in a light breeze*

Fig. 4 (b) shows the condensation on a cloudless night when the wind at 1 m seldom dropped below 3 m/sec. After sunset the rate of cooling was slow (about 0·5°C/hr) and the transition from evaporation to condensation was therefore a gradual one. Dew was first observed on the grass at 22 GMT and the trace shows that the pan began to increase in weight at about this time. At 0330 GMT cloud cover developed rapidly from zero to 7/8 and the subsequent rise in temperature explains why evaporation began before sunrise. The total dewfall for the night was 13 mg cm^{-2} compared with 8 mg cm^{-2} obtained by absorbing with filter papers the dew on nearby grass. The discrepancy is reasonable since the latter technique collects little of the moisture on the undersides of the leaves.

(c) *Condensation in calm conditions*

On clear ' radiation ' nights which are generally considered to be the most favourable for dew-formation, it is common for the wind in the lowest metre to drop below the stopping speed of a Sheppard cup anemometer, about 0·2 m/sec. Under these conditions, Rider and Robinson (1951) have suggested that the flow near the surface may be very nearly laminar and extremely small fluxes of water vapour would therefore be anticipated. Fig. 4 (c) shows a typical trace for such a night. At 21 GMT the rate of condensation was 5 × 10^{-8} g cm^{-2} sec^{-1} and the difference of absolute humidity between 1·0 and 3·5 cm (measured with fine-wire thermocouple psychrometers) was 2·9 × 10^{-7} g cm^{-3}. The diffusion coefficient calculated from these data is 0·43 cm^2 sec^{-1} which is only twice the value of the molecular diffusion coefficient.

The rapid cooling at sunset produced a well-marked termination of the evaporation and after a period of about an hour and a half when the flux was zero, condensation continued for the rest of the night at a rate of approximately 7 × 10^{-8} g cm^{-2} sec^{-1}. Thick fog developed during the night and retarded the onset of evaporation at sunrise.

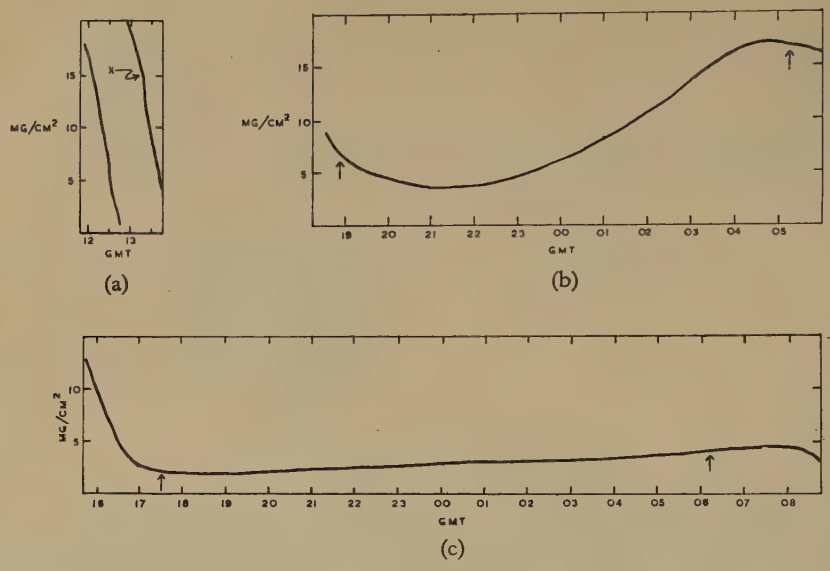

Figure 4. Change of weight per unit area of soil-pan with arbitrary origin. (a) 7 October, 1953; (b) 31 August to 1 September 1953; (c) 5 to 6 October 1953. (Arrows indicate the times of sunset and sunrise).

Although calm nights with large positive Richardson numbers are characterized by downward fluxes of very low order, filter-paper absorption on such nights shows that the rate of increase of water on the surface of the leaves is many times greater than the flux value. Two possible causes are being considered. One is distillation from warmer top soil to cooler grass; the other is condensation of water transpired by grass during the night.

6. Does dew rise or fall ?

The source of dew has long been a subject of controversy. Results from the present instrument indicate that since an increase of weight is recorded on all occasions of dew, at least a part of the moisture is produced by the condensation of atmospheric vapour brought to the surface by diffusion. This part may crudely be considered as having fallen. Provided the temperature of the leaves falls and remains below the dew point of the surface air, the rate of this dew*fall* is greater the higher the wind speed. On calm nights, however, dew*fall* is only a small fraction of the total condensation and since the remainder has come from the soil, either through the grass (transpiration) or direct from the soil surface (distillation), it may be said to have risen. It must be remembered, however, that since plant life makes an appreciable contribution to the water-vapour content of the surface layers of the atmosphere in daylight (as Fig. 4 (a) implies), the distinction between the two sources of dew is not a fundamental one.

Acknowledgments

We should like to thank Mr. B. J. Mason and Dr. H. L. Penman for their interest and encouragement and Mr. L. G. Morris of the National Institute of Agricultural Engineering for assistance with the calculations for the pivot system. One of us (J.L.M.) is indebted to the Department of Scientific and Industrial Research and to the Agricultural Research Council for grants held consecutively during the course of the work.

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A method for recording the formation and persistence of water deposits on plant shoots

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(Manuscript received 1 February 1954)

Summary

Apparatus has been designed to record the amount of water deposited on plant shoots by rain, dew and guttation, and how long the surfaces remain wet. These factors greatly influence the extent to which plants are infected by fungi causing such diseases as potato blight.

The water on a cut potato shoot, sealed into a water-filled chamber placed on a balance, can be weighed by recording, on a rotating drum, the changes in equilibrium of the beam. Deposits from rain appear rapidly; their persistence depends on the weather. In contrast, dew is deposited slowly over a long period and dries more rapidly. The heaviest dew deposit recorded was $6{\cdot}9 \times 10^{-3}$ g cm^{-2} compared with $9{\cdot}6 \times 10^{-4}$ g cm^{-2} for the amount of water retained during rain.

1. Introduction

Many micro-organisms require water deposits for movement and infection. The difficulty of determining the quantity and duration of such deposits has prevented sufficient study of their importance to plant pathology. The need for more information, particularly of the persistence of water deposits, has recently become apparent while studying the epidemiology of potato blight caused by the fungus *Phytophthora infestans*. The sporangia of this fungus are formed at night when the relative humidity is high; they are dispersed next day, mainly during the forenoon (Hirst 1953). They require water droplets for rapid germination and are killed by short exposure to moderate or low humidities. The percentage of sporangia which reach potato leaves and infect them may largely depend on the length of the period during which viable sporangia and water droplets co-exist. Infection by spores more resistant to desiccation may depend more on the length of the damp period during the succeeding night.

Rain, dew or guttation may deposit water on plant surfaces, but standard meteorological instruments provide data only of the time and amount of rainfall. The design of more informative apparatus is made difficult by such characters of plant surfaces as their colour, texture and attitude; also by vital processes such as transpiration and guttation (the excretion from leaves of water in liquid form) which make the use of replicas impractical. Preliminary tests showed that several grams of water are retained by the leaves of an average sized potato shoot when sprayed to ' run-off,' which suggested the practicability of weighing the deposits on a recording balance. Previous ' dew balances ' have used either an artificial surface unlikely to reproduce the conditions on a leaf (Stephan and Mildebrath 1938) or whole plants with roots and soil (Craddock 1951). In the latter type evaporation, transpiration, and drainage would produce changes in weight large compared with those due to the retention of rain water by the shoot, or the formation of dew and guttation droplets. Unwanted changes in weight must be reduced to a minimum.

2. The prototype balance

A prototype balance, shown diagrammatically in Fig. 1, was constructed to test the method and to suggest improvements in design. Obvious requirements for equipment that has to be placed in crops and exposed to the weather are robustness and low cost. The prototype was built round the beam from a commercial scale, sensitive to a change of load of less than 100 mg. On one side of the beam is hung a chamber, in the upper end of which a potato shoot, cut and fitted under water, can be sealed by means of a stuffing-box. This box contains two microcellular rubber washers separated by a layer of ' plasticine.' The upper part of the shoot is steadied by two stays rigidly attached both to the chamber and to the shackle which contains an agate bearing. From the bottom of the shoot chamber a flexible water-supply pipe of cycle-valve-tubing extends vertically as far as the level of the chamber's suspending knife-edge, thence along

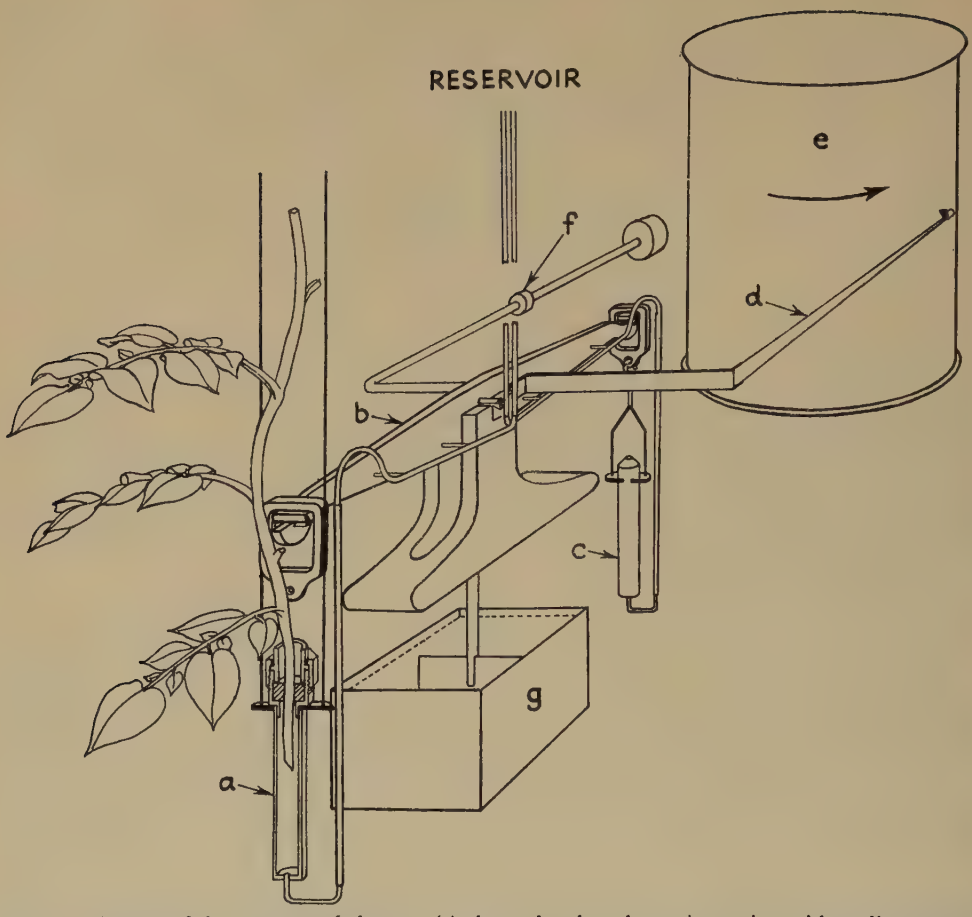

Figure 1. Diagram of the prototype balance : (a) shoot chamber shown in section with sealing materials cross-hatched; (b) beam; (c) compensating chamber; (d) pen arm; (e) clock drum; (f) counter-poise weight; (g) damper vane and tank.

the beam in a copper tube until it is in line with the central knife-edge, where it turns upward through another flexible connection to an open reservoir otherwise unattached to the beam. If the shoot is initially turgid and the system completely water-filled, transpired water will be made good from the reservoir without altering the equilibrium of the beam. The water supply is constant and unlimited, so that the shoot rarely wilts. Expansion caused by rising temperature however will force water up the pipe and into the reservoir, and make this side of the beam lighter. This change is counteracted by placing, on the other side of the beam, another chamber of the same volume, similarly connected to the reservoir; it is, of course, important that this compensating chamber should be exposed to the same temperature as the one containing the shoot. On the same side of the beam as the compensating chamber is attached a long pen arm, which traces a magnified record of the beam movement on an inverted standard daily clock drum. Also on this side of the beam, a movable counter-poise weight permits the pen to be set to the same zero position with shoots of differing weight. It is important that the apparatus influence the surrounding ecoclimate as little as possible, particularly that it should not affect wind speed and dew formation. The beam and recording drum should be below the leaves and the protecting cover, which should be made of materials that cool at approximately the same rate as the foliage. In the prototype the cover was of wood; unfortunately it was higher than desirable and somewhat impeded air-flow over the lower parts of the shoot.

The shoot grows too slowly to affect daily records, and diurnal changes from photosynthesis or respiration are too slight for the sensitivity of the beam. Wind, however, causes both a swing of the chambers about the shackles and a rapid oscillation of the whole beam. The oscillation is an advantage, because it gives a crude indication of turbulence and displays the frequent

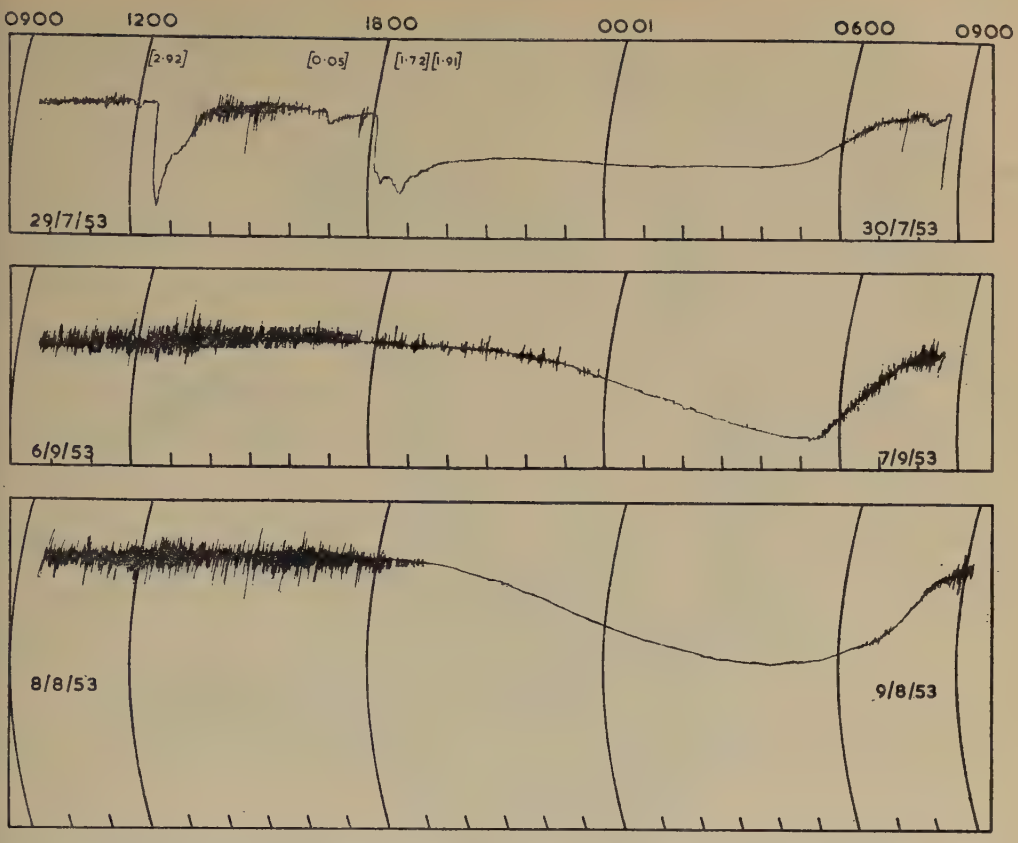

Figure 2. Daily records from the prototype balance : the figures in brackets are amounts of rain (mm) recorded each hour; for further explanation see text.

association of calm conditions and dew formation. The oscillation is damped by a vane immersed in oil. Apart from accidents, the only further changes of equilibrium experienced are those caused by water deposits.

3. Results

The apparatus was placed in potato crops in 1952 and 1953 with the top of the shoot level with the top of the crop. Individual shoots lasted in perfect condition for several days, usually about a week, thus permitting continuous recording. Photographs of three traces made by the prototype balance are reproduced in Fig. 2; a downward deflection of the trace indicates an increase in weight of the shoot. On days with neither rain nor dew, the beam maintains a steady average equilibrium about which wind may cause a rapid oscillation. The upper chart of Fig. 2 shows the deflection caused by 2·92 mm of rain falling between 12 and 13 GMT on 29 July 1953; rapid evaporation dried the leaves within two hours. Of much greater pathological significance is the overnight wetness of the leaves resulting from rain between 18 and 20 GMT on 29 July 1953. This period was calm but leaf surfaces sometimes remain wet for long periods despite considerable wind velocity. The rapid increase in weight of the shoot associated with rain contrasts sharply with the shape of the trace in the lowest two records, which were both made on nights of heavy dew. This slow and remarkably uniform rate of increase in weight of the shoot is typical of dew deposition; the variability in the rate of drying depends on the temperature, humidity and the wind velocity. The morning of 7 Sept. 1953 was warm and bright, whereas that of 9 Aug. 1953 was cool, overcast and less windy. The incidence of rain in the traces has been checked from a recording raingauge; for dew formation, checks have been obtained both by personal inspection and less directly by analysis of vapour-pressure profiles calculated at half hourly intervals from the records of a multiple wet- and dry-bulb temperature recorder (Hirst, Long and Penman 1954). Guttation is thought not to be such an important cause of surface water on potato leaves as it is on some other plants, for instance, the grasses.

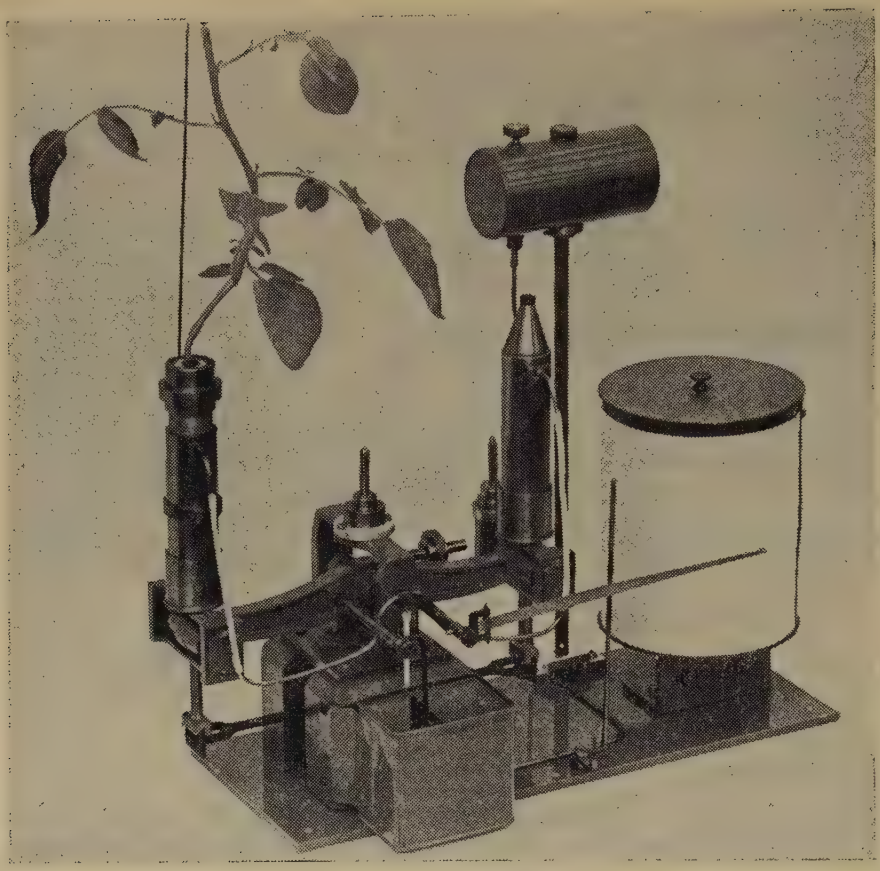

Figure 3. Photograph of the improved balance. The horizontal cylinder is the reservoir, and from it the water-supply pipe runs down to the base plate and then upwards to the chambers; the flexible valve-rubber connections are painted white.

4. Improved balance

Operation of the prototype balance suggested several improvements which have now been incorporated in an improved model shown in Fig. 3. The pendant chambers of the early balance were a disadvantage. The shoot was too low in relation to the beam and cover, and it had to be deflected to avoid fouling the shackle and knife-edge situated vertically above the shoot chamber. The safe load of the beam limited the size of the chambers, and the weight of the latter in turn limited the size of shoot which could be accommodated without becoming top-heavy. If, instead of a scale beam with suspended pans, a beam from a counter scale having fixed upright pans is used, then the shoot and compensating chambers may be placed above the knife edges. This greatly improves the exposure and allows shoots to be larger and more easily changed; it also prevents wind from swinging the chambers about the outer knife-edges. The beam in the balance shown in Fig. 3 is 9 in. long, has double knife-edges set 3 in. apart, thus giving good lateral stability, and has a safe load of 1 kg (W. & T. Avery Ltd., Type 2014). As purchased, the full swing of the beam was produced with a weight of about half that to be expected from the increase in weight on wetting a potato shoot; the sensitivity of the beam was therefore approximately halved by filing small amounts off the centre knife-edges. These changes of design meant that the water system had to be inverted, as will be seen by comparing Figs. 1 and 3. The reservoir is of course still raised to give a slight positive head at the top of the shoot chamber. Two sets of balance weights are now employed; for coarse adjustment a graded series of weights is added to the compensator end of the beam, and when not in use they are stacked on a spine on the central supporting frame of the beam; for finer balance there is a weight which can be moved along a screw-threaded arm attached to the centre of the beam. The paddle damper has been

moved to the front of the apparatus and its chamber supplied with a cover (not shown on the photograph). The pen arm is fitted on to a gate which allows the pressure exerted on the chart by the pen to be finely adjusted.

5. Discussion

This apparatus was constructed with the limited object of determining the length of time leaves were wet, but it could be used for other purposes such as measuring the amount of dew deposited, which would be valuable to agricultural meteorologists, particularly in arid zones. By measuring the surface area of the shoot and calibrating the deflection of the beam, the weight of water per unit area can be estimated. In 1953, one shoot was measured, and its surface area found to be 1,480 cm^2. Rain to ' run-off ' added 14·2 g, or $9{\cdot}6 \times 10^{-3}$ g cm^{-2}. The maximum increase in weight during the three dew nights while this shoot was exposed were 4·7 g, 7·0 g and 10·2 g. The weights of dew per unit of total surface area were 3·2, 4·7 and $6{\cdot}9 \times 10^{-3}$ g cm^{-2}, and the rate of deposition ranged from 6 to 8×10^{-4} g cm^{-2} hr^{-1} in the period between the start of dew formation and the time when it reached its greatest weight. Estimates of the mean weight of water over the total surface area of the shoot obscure the differences of distribution and persistence of water deposits at different levels, on the stems and petioles compared to the leaf surfaces, and on different parts of the leaf. These differences are probably very important to pathology; for instance, the frequent potato blight lesions on the tip of the leaflets are often associated with large persistent water droplets. They are more common after rain than after dew, which rarely approaches run-off under English conditions. Interesting observations might be made on these differences by the use of several balances bearing variously dissected shoots.

Acknowledgments

I wish to thank W. & T. Avery Ltd. for advice on the selection of beams, Mr. A. E. Hall who constructed the improved model, and Mr. V. Stansfield for Fig. 3.

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551.521.1

Solar radiation and the duration of sunshine

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Summary

Records of solar radiation (Q) and of duration of sunshine (n) on a monthly basis have been collected for 32 stations and regression equations relating the data calculated in the form $Q = Q_A (a + bn/N)$ where Q_A is the maximum possible radiation in the absence of an atmosphere, and N is the maximum possible duration of bright sunshine. For five of these stations records for long series of individual months have been examined and for all stations mean monthly values. In the overall regression equation $a = 0{\cdot}23$ and $b = 0{\cdot}48$.

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1. Introduction

There frequently arises in climatological studies a need for an approximate estimate of the solar radiation received at the surface of the earth. Such a need arose in the course of studies in crop physiology begun by one of us (J.N.B.) in association with Professor G. E. Blackman at Oxford and by the other two authors in connection with studies on evaporation in relation to meteorological factors in South Australia. That such an estimate could be obtained by a consideration of the relationship between recorded solar radiation at the earth's surface and the duration of sunshine was first suggested by Ångstrom (1924) and was expressed in the form,

$$Q = Q_0 [a' + (1{\cdot}00 - a') n/N] \quad . \quad . \quad . \quad (1)$$

where Q is the radiation actually received,

Q_0 is the total radiation income on a clear day,

n is the actual duration of sunshine received,

N is the maximum possible duration of sunshine,

a' is the mean proportion of the radiation received on a completely overcast day.

This proportion will vary from day to day and will for example be dependent on the density of the clouds. Ångstrom found that for weekly readings at Stockholm the value of a' was 0·25. Kimball and Hand (1936) subsequently proposed a value of 0·22 and Fritz and MacDonald (1949) have used the equation

$$Q/Q_0 = 0{\cdot}35 + 0{\cdot}61\, n/N$$

which allows for the fact that no recording instrument gives a full record of sunshine on cloudless days.

The same principle has been extended (Prescott (1940), Penman (1948)) to the relationship between the duration of sunshine and the theoretical amount of radiation to be expected if the atmosphere were perfectly transparent, using the values of Angot (Brunt (1939), p. 112). The equation becomes in this case :

$$Q = Q_A (a + bn/N)$$

$$\text{or} \qquad Q/Q_A = a + bn/N \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where Q_A = total radiation received if the atmosphere were perfectly transparent (Angot's value). When all four values of Q, Q_A, n and N are available the relationship expressed in Eq. (2) can be solved by calculating the appropriate linear regression for each set of data.

It is of interest therefore to extend the application of Eq. (2) to all data suitable for this purpose, leading, if possible, to an equation capable of general application in the calculation of radiation when the amount of sunshine is known.

The data used for establishing this relationship were of two kinds, firstly those in which monthly values for both measured radiation and sunshine were available for a number of months, in the examples quoted not less than 48, and where the Campbell-Stokes instrument was used for measuring sunshine, and secondly a wider group of observations where the 12 monthly values were available, each so far as possible the mean of records of at least three years.

TABLE 1. Relationship between solar radiation and sunshine calculated in terms of the equation $Q/Q_A = a + bn/N$ and based on individual monthly values.

Locality	Latitude	Number of months of observations	Period of observations	Mean values		Regression constants		Correlation coefficient
				Q/Q_A	n/N	a	b	r
Rothamsted[1]	51·8°N	84	1931-1940	0·371	0·358	0·18	0·55	0·79
Gembloux[2]	50·6°N	60	1939-1950	0·328	0·325	0·15	0·54	0·83
Versailles[3]	48·8°N	99	1935-1951	0·440	0·417	0·23	0·50	0·90
Mt. Stromlo[4]	35·3°S	144	1928-1939	0·593	0·631	0·25	0·54	0·89
Dry Creek[5]	34·8°S	48	1947-1950	0·600	0·591	0·30	0·50	0·95

Sources of data : [1] Calculated by Penman (1948) excluding Nov.-Feb.; [2] *Bulletin de l'Institut agronomique et des Stations de Recherches de Gembloux* : 1940-1951; [3] *Travaux effectués par les Stations agronomiques, Annales Agronomiques* : 1936-1952; [4] Rimmer and Allen (1950); [5] Solar saltfields of I.C.I. Alkali (Australia) Pty. Ltd.

2. Solar radiation and sunshine for selected individual stations

The stations selected for calculation of the appropriate regression equations were, in order of decreasing latitude, Rothamsted (England), Gembloux (Belgium), Versailles (France), Mt. Stromlo (A.C.T. Australia) and Dry Creek (South Australia). The relevant information and the constants of the regression equation are given in Table 1. The regression coefficient b is approximately constant at 0·50 and 0·55 and the values suggest no evidence of dependence on latitude. The values of the parameter a however show a marked trend, being smaller at the higher latitudes than at the lower ones. This may be associated in some measure with relative cloudiness.

3. Solar radiation and sunshine for a wider group of 32 stations

A second approach to the relationship between solar radiation and sunshine was to examine the data for as wide a range of stations as possible, on the assumption that any loss in precision resulting from the inclusion of data which are not strictly comparable is justified by the extension of the geographical coverage. Records of radiation and sunshine were obtained for 32 stations, the location and co-ordinates of which are given in Table 2, and the mean monthly values of Q/Q_A and n/N were calculated for each. Solution of Eq. (2) was then obtained on a total of 384 values, 12 for each of 32 locations.

TABLE 2. Stations for which solar radiation and sunshine data are available

Station	Country	Latitude	Source of data	
			Radiation	Sunshine
Batavia	Java	6·3°S	Boerma and Berlage (1948)	Schmidt (1950)
Tjibodas	Java	6·8°S	*Idem*	Braak (1923)
Bandung	Java	7·0°S	Dee and Reesinck (1951)	
San Juan	Puerto Rico	18·5°N	Hand (1949)	U.S. Dept. Commerce (1951)
Poona	India	18·5°N	Records of Central Agric. Met. Obs., Poona	
Honolulu	Hawaii	21·3°N	U.S. Dept. Commerce (1950)	U.S. Dept. Commerce (1951)
Miami	Fla., U.S.A.	25·7°N	Hand (1949)	*Idem*
New Orleans	La., U.S.A.	29·9°N	*Idem*	*Idem*
Fort Worth	Tex., U.S.A.	32·8°N	U.S. Dept. Commerce (1950)	*Idem*
Dry Creek	S. Australia	34·8°S	Records of I.C.I. Alkali (Aust.) Pty. Ltd.	
Albuquerque	N.M., U.S.A.	35·0°N	U.S. Dept. Commerce (1950)	U.S. Dept. Commerce (1951)
Mount Stromlo	A.C.T., Australia	35·3°S	Rimmer and Allen (1950)	
Nashville	Tenn., U.S.A.	36·2°N	U.S. Dept. Commerce (1950)	U.S. Dept. Commerce (1951)
Fresno	Cal., U.S.A.	36·7°N	*Idem*	*Idem*
Washington	D.C., U.S.A.	38·9°N	*Idem*	*Idem*
Columbia	Mo., U.S.A.	40·0°N	Hand (1949)	*Idem*
Pittsburg	Pa., U.S.A.	40·4°N	Hand (1937)	*Idem*
Salt Lake City	Utah, U.S.A.	40·8°N	U.S. Dept. Commerce (1950)	*Idem*
New York	N.Y., U.S.A.	40·8°N	*Idem*	*Idem*
Lincoln	Neb., U.S.A.	40·8°N	*Idem*	*Idem*
Chicago	Ill., U.S.A.	41·8°N	Hand (1949)	*Idem*
Boston	Mass., U.S.A.	42·4°N	U.S. Dept. Commerce (1950)	*Idem*
E. Lansing	Mich., U.S.A.	42·7°N	*Idem*	*Idem*
Madison	Wis., U.S.A.	43·1°N	*Idem*	*Idem*
Toronto	Ont., Canada	43·7°N	*Idem*	*Idem*
Versailles	France	48·8°N	Versailles, Official Records	
Gembloux	Belgium	50·5°N	Gembloux, Official Records	
Wahnsdorf	Germany	51·1°N	Goldschmidt (1952)	
Kew	England	51·5°N	Stagg (1950)	*Weather* (1951)
Rothamsted	England	51·8°N	Penman (1948)	
Stockholm	Sweden	59·4°N	Kimball (1930)	Ångstrom (1924)
Fairbanks	Alaska	64·8°N	U.S. Dept. Commerce (1950)	U.S. Dept. Commerce (1951)

It must be emphasized at the outset that no attempt has been made to treat the data in any other than a crude manner; while the most recent available records were always used, certain discrepancies have had to be accepted. In the first place, measurements have been made for varying periods at different stations : some values are the mean of up to 30 years for radiation

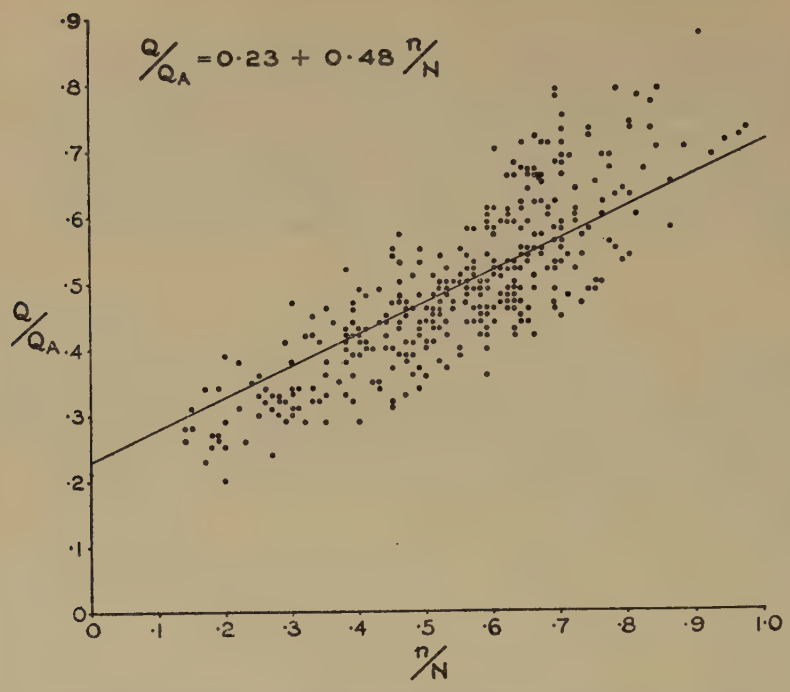

Figure 1. The relationship between Q/Q_A and n/N, using mean monthly values for 32 stations for which records of radiation and duration of sunshine are available.

and 60 years for sunshine and, so far as could be ascertained, no data of less than 3 years' duration have been included. In many instances, the recording of sunshine commenced before radiation, and as the records of individual years are not always available, the mean values have been used irrespective of the length of time to which they refer. This procedure will introduce inaccuracies caused by long-term fluctuations which may have affected values of sunshine in the years before the recording of radiation commenced. Secondly, measurements at different stations have been made with a variety of instruments. In the United States, the U.S. Weather Bureau and the Eppley pyrheliometers have been widely used for recording solar radiation, whereas in other countries the Moll-Gorczynski solarigraph, the Robitz actinometer, the Kipp solarimeter and other instruments have been employed. Similarly, the Marvin sunshine recorder has been in general use in the U.S.A. and the Campbell-Stokes elsewhere. In this connection it is of interest to note that Brooks and Brooks (1947) in a comparative study of the last two instruments, found a difference in the level of insolation at which recording begins. No attempt has been made to reduce the data obtained by different instruments to a common standard.

The relationship between the mean Q/Q_A and n/N values thus obtained is shown graphically in Fig. 1. Although a good linear relationship was immediately apparent, it was considered advisable to submit the data to statistical analysis to determine how much, if any, of the observed variability could be accounted for by trends in season or latitude.

It was found on analysis that the 32 stations could be arranged in seven groups, each group having regression coefficients significantly different from its predecessor. This grouping and relevant regression constants – calculated on the residual variability after association due to months and stations with groups had been eliminated – are given in Table 3. Although the values of the regression coefficient b vary from 0·280 to 0·613, there is no evidence of a consistent trend and in view of the high correlation between Q/Q_A and n/N it was thought advisable to ignore the differences between groups and to derive a single regression equation relating all the Q/Q_A and n/N values. This regression equation was again calculated on the residual variability after association due to months and stations had been eliminated, since this procedure was considered to be the most valid basis for prediction, and is shown in Fig. 1. The regression is

$$Q/Q_A = 0{\cdot}23 + 0{\cdot}480\, n/N$$

TABLE 3. ARRANGEMENT OF STATIONS INTO GROUPS WITH SIGNIFICANTLY DIFFERENT REGRESSION COEFFICIENTS

Group	Station	Range of latitudes (degrees)	Regression constants	
			a	*b*
1	Batavia, Tjibodas, Bandung	6·3- 7·0	0·29	0·290
2	San Juan, Poona, Honolulu	18·5-21·3	0·27	0·613
3	Miami, New Orleans, Fort Worth, Dry Creek, Albuquerque, Mt. Stromlo	25·7-35·3	0·40	0·274
4	Nashville, Fresno, Washington, Columbia, Pittsburg, Salt Lake City, New York, Lincoln, Chicago	36·2-41·8	0·20	0·471
5	Boston, E. Lansing, Madison, Toronto, Versailles, Gembloux	42·4-50·5	0·30	0·280
6	Wahnsdorf, Kew, Rothamsted	51·1-51·8	0·19	0·567
7	Stockholm, Fairbanks	59·4-64·8	0·22	0·521

These values of the regression constants compare favourably with those obtained from the more precise data available for the five stations quoted in an earlier section of this paper, and it is felt that this equation is capable of general application, at least within the range of latitudes studied. As further records of solar radiation and sunshine become available it will be possible to increase the accuracy of prediction by the calculation of regression equations based on long-term records of a wider range of stations, and with the possibility of separating the records associated with the Campbell-Stokes sunshine recorder from those of the Marvin instrument.

ACKNOWLEDGMENTS

Our thanks are due to Mrs. Irena Mathison for advice, statistical assistance and for checking the calculations.

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551.573

A note on the estimation of diurnal amounts of evaporation

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(Manuscript received 4 May 1953, in revised form 2 January 1954)

Pasquill (1949) has considered the possibility of using daily mean values of the vapour-pressure difference between two levels (1 and 2) at heights of 25 cm and 50 cm respectively above soil surface, and of the wind speed at level 2, for computing the daily amount of evaporation from an aerodynamically rough pasture surface. He writes the Thornthwaite-Holzman (1942) equation in the simplified form

$$E = A\, u_2\, (e_1 - e_2) \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (1)$$

where E is the rate of evapotranspiration, u_2 the wind speed at level 2 and e_1 and e_2 are the values of vapour pressure at levels 1 and 2 respectively. A is a constant dependent primarily on the roughness of the surface.

Pasquill states that the use of daily mean values of u_2 and $(e_1 - e_2)$ to compute from this equation the daily amount of evaporation may lead to considerable error and points out that if the diurnal cycles of wind speed and vapour-pressure difference follow simple sine waves, in phase with one another (a reasonable approximation), with amplitudes a and b respectively,then

$$\overline{E} = A\, \overline{u}_2\, \overline{(e_1 - e_2)} + \tfrac{1}{2} Aab \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

where the symbols with bars represent daily mean values. With amplitudes a, b of the same order as the daily means, as in Pasquill's data, the second term in Eq. (2) is of the order of 50 per cent of the first term, or

$$\overline{E} \simeq \tfrac{3}{2} A\, \overline{u}_2\, \overline{(e_1 - e_2)} \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (3)$$

We proceed to enquire whether reasonably correct values of $\overline{E}$ can be deduced from Eq. (1) by substituting on the right-hand side, instead of the daily means, $\overline{u}_2\,(1 + \alpha)$ and $\overline{(e_1 - e_2)}\,(1 + \alpha)$ respectively. It is easily seen that (1), thus modified, and (3) give the same value for $\overline{E}$, provided $\alpha = 1/4$ (more precisely, if $\alpha = 0{\cdot}225$).

Again, assuming as before a sinusoidal variation for u_2 [and $(e_1 - e_2)$], with a maximum at noon and an amplitude equal to the daily mean value, if 2τ be the length, in radians, of the portion of the diurnal cycle which has the mean value $\overline{u}_2\,(1 + \frac{1}{4})$, then

$$\frac{2\int_0^{\tau} (\overline{u}_2 + \overline{u}_2 \cos t)\, dt}{2\tau} = \tfrac{5}{4}\, \overline{u}_2 \qquad . \qquad . \qquad . \qquad (4)$$

Upon integrating, we are led to an equation whose relevant positive solution is $\tau \simeq \frac{2}{3}\pi$, or τ corresponds to about 8 hr. Hence $\frac{5}{4}\,\overline{u}_2$ is the mean value of u_2 over a period of some 16 hr centred at noon. This is just about the duration of daylight, sunrise to sunset, for the latitude (*c.* 52°N) and season (May 10) at which Pasquill's observations were made.

The implications of the above result are in agreement with the observation that evapotranspiration is primarily a daylight-hour process. At night, wind speed is so low, as is also the vapour-pressure difference in the air, that the evaporation-condensation contribution of the hours of darkness is practically negligible (Halstead 1951). Therefore, if Eq. (1) is to be used for finding the daily mean rates of evapotranspiration, mean values of u_2 and $(e_1 - e_2)$ for the daylight hours should be substituted.

We have evaluated from Pasquill's graphs daily mean values of u_2 and $(e_1 - e_2)$ between 0415 GMT (sunrise) and 1935 GMT (sunset) and inserted them in Pasquill's equation :

$$E = 4{\cdot}96 \times 10^{-8}\, u_{50}\, (e_{25} - e_{50})$$

(E in g cm^{-2} sec^{-1}, u in cm sec^{-1} and e in mm Hg.). Multiplying the results by 55,000, the approximate number of seconds between sunrise and sunset on May 10, at the latitude of Pasquill's station (Girton Pasture), the following figures are obtained :

TABLE 1. COMPUTED DAILY WATER LOSSES (IN MM)

Day in May 1947	From daily mean data (Pasquill)	From hourly data (Pasquill)	From daily mean daylight-hour data
8	1·5	2·2	2·3
9	1·8	3·6	3·6
10	1·0	1·4	1·4
11	2·7	2·5	2·7
12	1·7	2·1	2·0

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551.508.77

A comparison of the Dines and Bibby raingauges

By G. SPURR

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(Manuscript received 9 October 1953, in revised form 17 December 1953)

SUMMARY

The Dines and Bibby raingauges were designed to fulfil different purposes; one to measure total rainfall, the other to measure rate of rainfall. The two instruments have been compared to find out whether each would carry out the function of the other.

1. INTRODUCTION

From 1951 to 1953, the Dines and Bibby raingauges have been recording rainfall alongside each other at the Transmission Line Test Site at Leatherhead.

A study of the relationship between weather and overhead line power losses required the continuous measurement of rainfall amounts and rate of rainfall. Since on some occasions one instrument was recording without the other, it was considered worth while making a comparison of the behaviour of the two instruments.

The Bibby raingauge was designed to measure rates of rainfall, but by using a Post Office counter it was modified to count the total number of drops produced by a rain shower. The equivalent rainfall amount represented by one drop was found to be 0·03 mm. This was measured by pouring a known volume of water drop by drop into the funnel. Knowing the area of the funnel's mouth, and the total number of drop impulses produced, the equivalent rainfall of one drop could be calculated. The rate of rainfall was found from the Dines record by measuring the slope of the trace. Gradients were measured which coincided with the maximum rates of rainfall recorded by the Bibby.

2. RESULTS

In Fig. 1, records of rainfall amounts are compared. For fifty-one pairs of readings the correlation coefficient was 0·97 and, as can be seen in the figure, the two regression lines are very close together.

	Dines (mm)	Bibby (mm)
Mean rainfall in a period of rain	5·3	5·5
Standard deviation about the mean	5·8	6·1
Correlation coefficient	0·97	
Regression lines	$D = 0{\cdot}92\,B + 0{\cdot}2$	$B = 1{\cdot}03\,D + 0{\cdot}05$

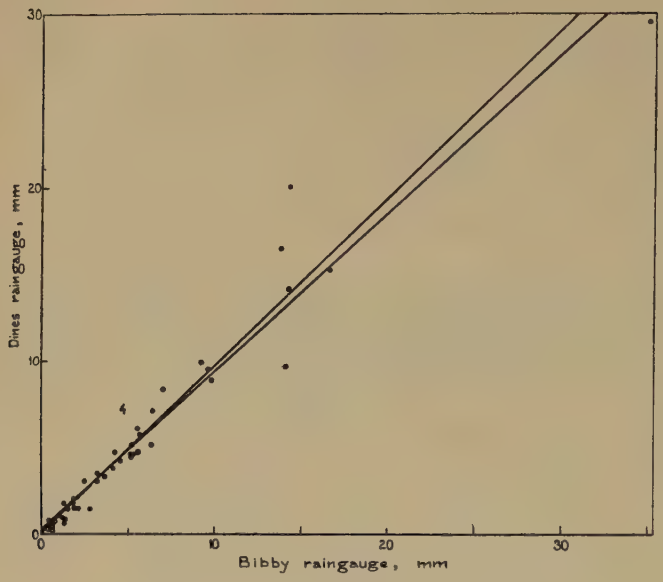

Figure 1. Rainfall amounts.

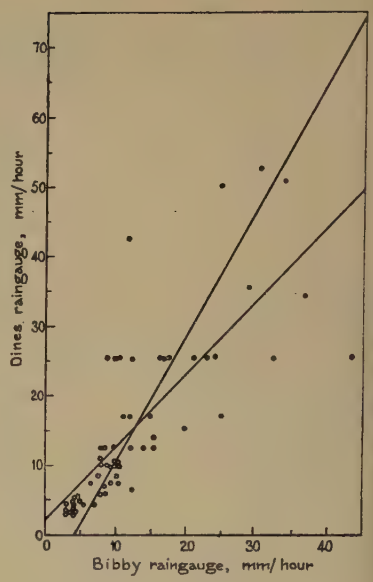

Figure 2. Rate of rainfall.

It seems likely that the Dines raingauge records about 0·95 the amount of rainfall recorded by the Bibby in a given period of rain.

Comparison of rates of rainfall is made in Fig. 2. Here there is a wide scatter of points, which is to be expected in view of the difficulty in measuring the slope of the Dines trace. Nevertheless, in sixty-three pairs of readings the correlation coefficient of 0·76 has some significance. Since the Bibby is the more accurate in measuring rates of rainfall the regression equation

$$d = 1{\cdot}03\,b + 2{\cdot}5$$

will give the best relation between the two sets of readings.

	Dines (mm/hr)	Bibby (mm/hr)
Mean of rates of rainfall	15·6	12·8
Standard deviation	12·2	9·0
Correlation coefficient	0·76	
Regression lines	$d = 1{\cdot}03\,b + 2{\cdot}5$	$b = 0{\cdot}56\,d + 4{\cdot}06$

3. Conclusions

From these results it can be seen that there is good agreement between the two instruments for measurements of total rainfall. The small differences between recordings may be due to a slight inaccuracy in accepting 0·03 mm as the equivalent rainfall for one drop; or it may be due to evaporation taking place from the wide funnel of the Dines gauge.

The differences in the recording of rates of rainfall made evident by Fig. 2 may be explained by the difficulty involved in measuring the slope of the Dines trace.

It is worth noting that high rates of rainfall appear to last for only a short time. Measurement by the Dines is very difficult owing to the steepness of the slope; also, intervals of three minutes in the Bibby recorder may be too long to measure the high rate without including slower rates of rainfall in the interval.

4. Acknowledgment

This note is published by permission of the Director of the Research Laboratories, British Electricity Authority.

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551,501.7 : 551.557

Upper winds over the World

By E. Knighting

There are two papers with the title 'Upper winds over the World'; the first is in Vol. **72** of the *Quarterly Journal* (1946, p. 55), the second is *Geophysical Memoir*, No. 85 and they are referred to in the sequel as *Q.J.* and *G.M.* respectively. The comments made here fall in two parts, firstly a discussion of the relation between V_r, V_s and σ and secondly a numerical check of the accuracy of a computed wind rose. (For definition of symbols see *G.M.*, p. 49).

It is somewhat astonishing to find that the symbol Q defined by the equation following (7) on p. 62 of *Q.J.* should have led to the various approximations which are then discussed, since Q is simply given by

$$Q = \frac{2}{\pi}\left(1 + \frac{v}{V_r}\right) \mathrm{E}\left(\frac{2\,(v\,V_r)^{\frac{1}{2}}}{V_r + v}, \frac{\pi}{2}\right)$$

and the elliptic function is exhaustively tabulated. It is clear, also, that the expansions given on p. 4 of *G.M.* are incorrect and should read $x^4/64$ and $1/(64\,x^3)$ in the third terms.

Q itself is unnecessary in giving a relation between V_r, V_s and σ and gives rise only to a large amount of numerical computation which can be avoided by using the equation at the top of p. 62 of *Q.J.* in the form

$$V_s = \frac{1}{\pi\sigma^2}\int_0^{\infty}\int_0^{2\pi} V^2 \exp\left\{-\frac{1}{\sigma^2}(V^2 + V_r^2 - 2VV_r\cos\phi)\right\} dV\,d\phi;$$

the integration is carried out first with respect to ϕ and then to V, giving

$$V_s = \frac{\sqrt{\pi}\sigma}{2}\,{}_1F_1\left(-\tfrac{1}{2}, 1, -\frac{V_r^2}{\sigma^2}\right)$$

where ${}_1F_1$ is the confluent hypergeometric function, tabulated in *British Association Tables*, 1926. If the tabulation had not already been carried out, the last equation would still be far superior to those of either *Q.J.* or *G.M.* both in accuracy and saving of labour.

Turning to the numerical side, the calculation of a wind rose from the tables given by the authors involves interpolation within one and possibly two tables, enough numerical work to make an independent check desirable. The final analysis will lead to a table such as Table VI of *G.M.* The check proposed here is upon the entries in the column headed 'all directions' and the row 'all speeds.'

The frequency for all directions and between speeds 0 and V is

$$F_V = \frac{1}{\pi\sigma^2}\int_0^{V}\int_0^{2\pi} \exp\left\{-\frac{1}{\sigma^2}(V^2 + V_r^2 - 2VV_r\cos\phi)\right\} V\,dV\,d\phi$$

$$= \frac{2}{\sigma^2}\int_0^{V} \exp\left\{-\frac{V^2 + V_r^2}{\sigma^2}\right\} I_0\left(\frac{2VV_r}{\sigma^2}\right) V\,dV.$$

Putting $\frac{V^2}{\sigma^2} = w$, $\frac{V_r}{\sigma} = \alpha$ and expanding the Bessel function gives

$$F_V = \exp(-\alpha^2) \sum_{n=0}^{\infty} \frac{\alpha^{2n}}{(n\,!)^2}\int_0^{V^2/\sigma^2} e^{-w}\,w^n\,dw.$$

Since n is an integer, the integrals can very easily be written down, and the presence of $(n\,!)^2$ in the denominator ensures that not too many terms are necessary. Thus F_V is easily found and used to check the final entries in the 'all-directions' column. If V^2/σ^2 is large it is better to use $(1 - F_V)$, which gives the sum from the bottom of the column, and change the limits of the integral for F. The q-tables of *G.M.* and *Q.J.* can themselves be tested, e.g., in Table XXVIII of *G.M.* if V/V_r is taken as 2, three terms of the series are quite adequate and give $F = 0{\cdot}5329$; summing the first five entries in the column 'all directions,' gives 0·5376, a remarkable agreement.

The check for F_V will go far towards checking the numerical accuracy; an additional check exists for the frequency F_ϕ between two values of ϕ for all values of V. Then, putting $V_r/\sigma = \alpha$

$$F_\phi = \frac{1}{\pi\sigma^2}\int_{\phi_1}^{\phi_2} d\phi \int_0^\infty \exp\left\{-\frac{1}{\sigma^2}(V^2 + V_r^2 - 2VV_r\cos\phi)\right\} V\,dV$$

$$= \frac{1}{\pi}\int_{\phi_1}^{\phi_2}\left\{\tfrac{1}{2}\exp(-\alpha^2) + \frac{\sqrt{\pi}}{2}\alpha\cos\phi\,\mathrm{erfc}(-\alpha\cos\phi)\exp(-\alpha^2\sin^2\phi)\right\} d\phi.$$

If $\phi_2 - \phi_1 = 20°$ it will generally be sufficient to approximate to the integral by using

$$F_\phi = \tfrac{1}{18}\left\{\exp(-\alpha^2) + \alpha\sqrt{\pi}\cos\overline{\phi}\,\mathrm{erfc}(-\alpha\cos\overline{\phi})\exp(-\alpha^2\sin^2\overline{\phi})\right\}$$

where $\phi_1 + \phi_2 = 2\overline{\phi}$.

Thus taking Table XXVIII of *G.M.* the row 'all speeds' gives:

114·3 100·4 79·4 58·8 43·3 32·7 26·1 22·2 20·5

However, most practical applications will deal with $\phi_2 - \phi_1 = 45°$, which gives for Table VI of *G.M.* the row,

117·5 62·3 45·1 49·4 80·1 160·8 254·4 224·5

hence providing a check on that table.

The application of these two checks, which is quite rapid, should ensure that the numerical results obtained by interpolation are correct.

Meteorological Office,
Shoeburyness, Essex.
20 January 1954.

551.593.12

Stellar scintillation

As already reported on p. 116 of this volume, a joint discussion meeting with the Royal Astronomical Society was held under the above title on 16 December 1953. The opening papers and general discussion are given below.

551.593.12

Observations of stellar scintillation

By H. E. BUTLER
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1. Introduction

The naked-eye phenomenon of the scintillation or twinkling of stars is familiar to everyone. On a completely clear night, the brightness of a star 20°, say, above the horizon, instead of appearing constant, is observed to fluctuate in an apparently random manner. We cannot correctly speak of the *frequency* of such variations, but we can normally estimate that the time interval between moments of maximum brightness is between 1/10 and 1 sec.

Further, as we examine stars at successively greater elevations in the sky, we note, again on a normal night, that the amount of scintillation – that is the amplitude of the brightness variations – decreases until, above an elevation of 40° or 50°, hardly any is visible. On the other hand, we see that the scintillation of stars near the horizon is greater than of those at 20°, and in addition we usually notice that the intensity varies more slowly.

Now evidently these observed intensity variations might be the results of any of the following :

(a) The variations are not real, being the result of the physiology of the eye or brain.
(b) The star itself is producing the variations.
(c) The variations are caused in interstellar space.
(d) The variations are caused by the earth's atmosphere.

The first of these points received much publicity four years ago*, and need not be discussed further here, particularly as there are now abundant instrumental recordings of scintillation to show that the major part, at least, is not physiological. The other three points need just a little more examination. If, during one night or a series of nights, we watch a particular star from when it is near the horizon until it is nearly overhead, we can usually say that while *its* scintillation depends on *its* elevation in the sky, at the same time from observation of all the other stars, the scintillation *in general* has not altered appreciably. Thus by far the greater part of the effect is a function of altitude and is therefore, in all probability, due to our own atmosphere.

In spite of the fact that very little visual scintillation can be seen for stars near the zenith, instruments show that it is still present. As a result of this and since no suitable observations have yet been made from outside the atmosphere, we cannot state with certainty either that no interstellar scintillation exists or that some of the rapid fluctuations in intensity may not be due to the stars themselves. There is reason for believing, however, that such effects, if they exist, will not exceed a few per cent. We will here, at any rate, assume that all scintillation is due to our own atmosphere.

2. Instrumental recordings of scintillation

When we wish to make recordings of scintillation, we do so in the obvious way. A telescope is pointed at a star and, at a point near the focus, the star-light is directed in some suitable way on to the cathode of a photo-multiplier cell. The output current from the cell is, of course, directly proportional to the intensity of the star-light falling on it and can be recorded as we wish. Fig. 1 shows such recordings for stars at three different elevations and illustrates the

* See *Nature*, 1950, **165**, pp. 146, 663, 1019 and **166**, p. 151; *Observatory*, 1950, **70**, p. 60.

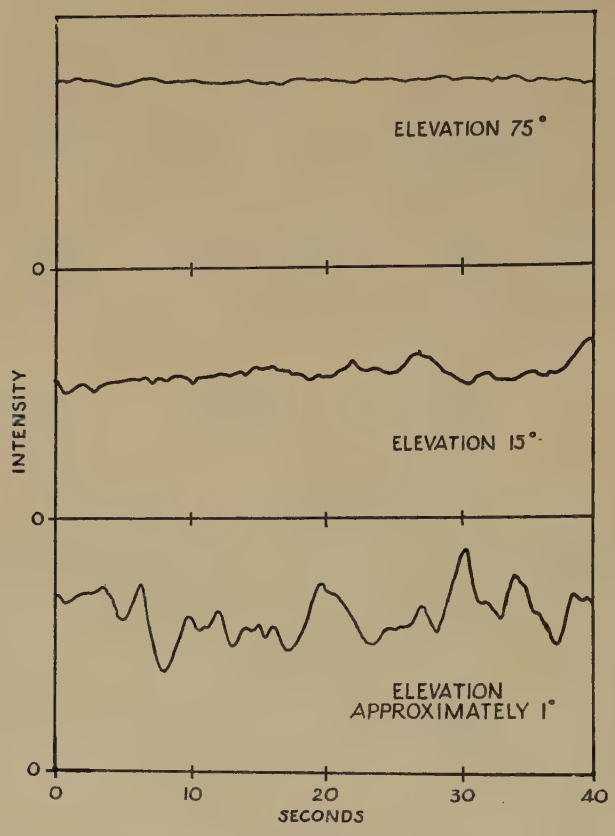

Figure 1. Recordings of stellar scintillation at different elevations. Instrumental time constant is of the order of one second.

description given earlier for stars of high, medium and low elevation. All were taken on one night (15 Sept. 1950) with the 15-in. reflecting telescope of Dunsink Observatory, Dublin. Intensity is measured vertically while the time axis is horizontal. The photo-current was measured with a galvanometer which had a time constant of about one second and its ability to respond to the fluctuations is evidently similar to that of the human eye.

Such a method of recording is obviously very limited by this slow response of the galvanometer and Fig. 2 shows three similar records made using a cathode ray oscilloscope – the instrumental time constant now being of the order of one millisecond. Again the recordings were all made on one night (2 April 1951) with the 15-in. reflector. Extremely large and rapid fluctuations in intensity missed by both the eye and the galvanometer are clearly evident, it being possible to count a hundred or more peaks per second. Such records have now been made and published independently by many people particularly, Nettleblad (1951 and 1953), Ellison and Wilson (1951), Ellison and Seddon (1952) and Butler (1950-1952).

Fig. 3 shows a record made in the same way as those of Fig. 2 except for a much extended time scale and with the telescope aperture reduced to 4 in. The very sharp pulses rising to 300 or 400 per cent of the mean are not exceptional. On the other hand, records made during a hazy night have been very steady and have borne out the general astronomical observation that good seeing and haze go together.

3. Indirect observations of scintillation

Scintillation shows itself in other ways, however, than those just described – ways which show us a little more of what is happening. When, for instance, we look at a star in an astronomical telescope, it is rarely that we see the central bright spot surrounded by the diffraction rings that theory predicts. In general, we see a hazy image which may be as much as ten or a hundred

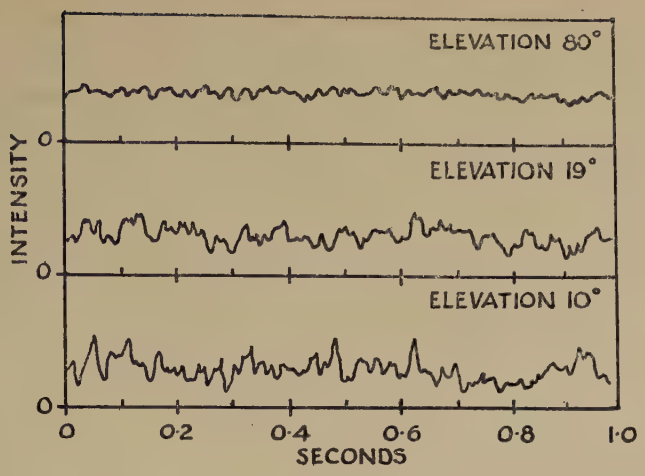

Figure 2. Recordings of scintillation at different elevations. Instrumental time constant is of the order of one millisecond.

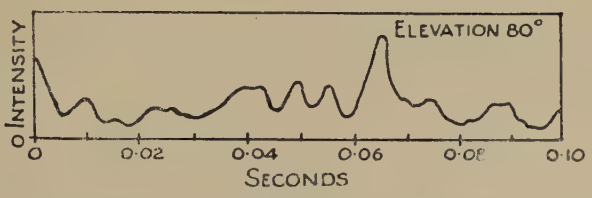

Figure 3. Recording of scintillation using expanded time scale and telescope aperture of 4 in. Instrumental time constant is of the order of one millisecond.

times the theoretical diameter and over which the distribution of light is neither constant nor uniform. In addition, the whole image is frequently to be seen in continual motion at right angles to the line of sight, the amount of the movement varying with the seeing conditions and the aperture of the telescope. The sharpness and steadiness of such an image – the two points that together make up what we term astronomical 'seeing' – have been studied using small mobile telescopes in the United States and elsewhere when prospecting for new observatory sites (Anderson 1942). It is interesting to note that seeing conditions can change with remarkable speed. In South Africa, in particular, I have seen a stellar image grow in a matter of a few seconds to two or four times its previous diameter.

Consider a beam of star-light of small cross-section as it passes through the atmosphere. First, there will be the normal refraction effect which will bend the beam by an amount depending on the altitude of the star, and which is a maximum of about 35′ at the horizon. As far as we are concerned at the moment, we will ignore this effect. What interest us are the deviations about this mean line caused by the variations in refractive index. Such variations can arise from a variety of causes – variations in density due to local temperature differences or to turbulence, non-uniform distribution of water vapour in the air, and so on – and the inhomogeneities can obviously exist anywhere throughout the atmosphere. Thus any beam will describe a tortuous path about the mean path. A second elementary beam, contiguous to the first when outside the atmosphere will, to a certain extent pass through the same inhomogeneities and so will be deviated along with its neighbour, and so remain 'parallel' to it. On the other hand it will evidently not entirely do so and will, in general, strike the ground a little distance from where the first one does, and similarly for the other beams. Evidently, then, if we speak of the illumination of the ground by just one star, we have to picture the result of all these individual beams lighting up their own patches of ground. Some will remain contiguous and give the uniform brightness that we expect. Some will overlap and cause a bright patch and, of course, when this happens, there will be other areas getting less light than the average. It is important to realize that the deviations to the beams are mostly angular, and very small. On a night of average

seeing, a telescopic star image will be of the order of a few seconds of arc in diameter – in other words, after all the random deviations suffered by each element making up the telescope beam, the majority will still be making an angle of less than one second of arc with the mean direction.

It is unnecessary to say that the atmospheric irregularities are anything but stationary. They will be forming and dispersing, moving with their own individual motions with respect to the neighbouring air and at the same time the whole will be moving along with the winds. Thus the appearance of the pattern at the ground caused by a single star will continually be altering.

Such patterns can occasionally be seen under freak conditions. The one most frequently quoted occurs during the seconds just before and just after a total eclipse of the Sun, when only a minute portion of the Sun, approximating to an intensely bright point source, remains uncovered by the Moon. The ground is then seen to be covered by a complex, wavy, mottled pattern in rapid movement. Such patterns can also be seen occasionally by night when a distant aircraft flare chances to illuminate a suitable surface near the observer.

But there is a much easier way by which an astronomer can view the illumination caused by a single star. Let him point a telescope at a star – a reflecting telescope, say. Then if the eye is placed at the stellar image (using no eye-piece) and the telescope mirror be examined, only the light from the chosen star will be reflected into the eye and he will see the non-uniformity of the light falling on the mirror. Photographs of this are to be seen in an excellent paper by Mikesell, Hoag and Hall (1951).

As we watch this pattern changing on a telescope mirror, whilst one definitely appreciates the continual change and movement of the pattern, one also gets the impression of a regular waviness. This is very difficult to describe and frequently it is even difficult to convince oneself of the direction in which the pattern is moving. The apparent regularity of the pattern has caused Gaviola (1949) and others to maintain that scintillation is caused by refractions at localised regions of waves – pressure waves, presumably – in the atmosphere, situated at the interfaces of layers of air of different temperatures, which may be moving across each other at different speeds. Whilst I certainly agree that such effects can occur and are to be seen, I feel it a much more satisfactory explanation in general that the pattern is the result of turbulence and similar effects extending either throughout the whole atmosphere or throughout thick layers of it. I prefer to explain the waviness of the pattern as due either to the wind blowing the atmospheric irregularities across the beam, or, possibly, to the average irregularities instead of being spherical, being cylindrical, with axes related to the wind directions. Persistence of vision and length of photographic exposure will both, of course, also tend to draw out the bright and dark patches along the direction of the wind. The paper, by Mikesell, Hoag and Hall (1951) also shows cine-photographs of a telescope mirror in the light of a single star and the elongation of the patches can be seen along the direction of the wind.

One further point in connection with the lifetime of the patches of brightness in such ground illumination may be of interest. When working on stars I also made a series of observations and recordings on distant street lights where incidentally, the streaks or waves to be seen on the telescope mirror are all horizontal, which is what we should expect if they were the result of the wind. In the first experiment, I placed a photo-cell near the focus of the telescope as before, only this time I diaphragmed down the objective to two parallel slots perpendicular to the waves on the mirror. The resultant scintillation trace shows some of the sharp peaks as doubled – that is, a small bright area remained bright as it moves across first one and then the other slot. On the other hand, some sharp peaks were not doubled, implying that they appeared or disappeared in the few inches between the two slots. No doubling was evident when the slots were parallel to the direction of the wind.

In a second experiment, I recorded an image of the telescope mirror on continuously moving photographic film. Here also I obtained evidence that the bright areas appeared and disappeared with great rapidity.

In general these, as well as other recordings of scintillation, are very difficult to analyse and to interpret. Mention must again be made of the work by Mikesell, Hoag and Hall (1951) in which they feed the output of a photo-cell into frequency-sensitive circuits and so make a direct harmonic analysis.

It will be noted that I have given the impression that refraction is the only effect causing scintillation. That is only because I find it easier to picture what is happening in this case than I do when diffraction has also to be considered. The diffraction aspect of scintillation has been considered in recent papers by Little (1951) and Chandrasekhar (1952). The former finds that smaller atmospheric density gradients are needed to produce much the same effect as in the case of refraction.

4. The effect of scintillation on astronomical work

As far as the astronomer is concerned, scintillation is always a nuisance. Stellar images that should be 1/10 or 1/50 of a second of arc in diameter are 10 or 100 times greater. As a result, quality is lost in the photography of both stars and planets while in the former case we also have to make a longer exposure. Similarly in spectroscopy, since the stellar image on the spectrograph slit is so large, most of the star-light is lost and exposures have to be 5 or more times the duration they otherwise would be. Many devices have been and are being tried to reduce this effect – such as, in the case of direct photography, the automatic movement of the plate itself in an attempt to keep up with the sideways movements of the stellar image. Similarly, in the photography of planets, cinematography is used in the hope that, out of many thousands of photographs, a few will have been made when the atmosphere throughout the light beam chanced to be static.

Needless to say, if we knew more about the mechanism causing scintillation, we would know which of these attempts were forlorn hopes and those with which we should persevere. Like everyone else, we want to know the height of the most troublesome disturbances and, in particular, what proportion of the effects take place within a few hundred feet of the dome.

5. Conclusion

We are surprisingly short of even the simplest observations, particularly standardized observations extending over a large area. My only attempt at obtaining more of these was made when I was in Dublin. I encouraged a group of interested amateur astronomers to note at two-hourly intervals each night, the highest star in the sky that was visibly scintillating. The results were discordant : I am wondering to what extent the experiment might have succeeded if it had been carried out by skilled observers.

Would the meteorological service be interested in starting such observations ? If small telescopes were available, it would also be possible to make routine observations of stellar image diameter and lateral movement, such as those that were mentioned earlier.

As regards controllable experiments, as far as I am aware, records are non-existent. It would seem most important to obtain scintillation records using an artificial source and a relatively short path. The amplitude of the beam as measured with a photo-cell could then be correlated with turbulence, etc., as measured with ultra-sensitive barographs, or otherwise. The records could easily be extended by using two or more parallel telescopes. The correlation of the records from these would then indicate the size and the lifetime of the atmospheric irregularities.

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Location, size and speed of refractional irregularities causing scintillation

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1. Introduction

The effects of atmospheric turbulence upon star images may be classified under three headings :

(a) Changes of apparent brightness (brightness scintillation).
(b) Changes of colour (colour scintillation).
(c) Changes of position, including ' dancing ' and ' pulsation.'

Dr. Butler has dealt fully with (a); brightness scintillation is ideally suited to investigation by means of the photoelectric scintillometer. I wish to treat certain aspects of (a), (b) and (c) which are primarily the concern of meteorologists.

Essentially what we observe in (a), (b) and (c) are the results of *refractional irregularities* occurring in the atmosphere. Can we localize these turbulent elements at one, or more, definite atmospheric levels, to be identified perhaps with inversion layers ? Can we measure the average dimensions of the turbulent elements ? Can we measure their velocities of drift relative to the earth's surface ? Answers to such questions are of great interest, I take it, to meteorologists.

The photoelectric technique, as applied to stars (point sources), with high time resolution provided by means of a cathode ray tube, gives valuable information about the brightness changes, but tells us little about the average sizes of the refractional irregularities causing them. Photoelectric observations of planets (extended sources) can be used to investigate the angle subtended at the telescope by these turbulent elements. The light from each point of a planet's disk scintillates precisely like a star at the same altitude. But, if the planet's disk subtends an angle large relative to that of the turbulent elements through which the light has to pass, each point on the disk will scintillate out of phase with neighbouring points and the total light from the planet will be steady (Ellison and Seddon 1952).

When we compare the scintillation amplitudes of Jupiter (subtending 49″), Jupiter's third satellite (subtending 1″.5) and a star, all at the same altitude, we find that Jupiter III scintillates precisely like a star, whereas the light of Jupiter is perfectly steady. Now, if we choose for observation a planet whose disk subtends the same angle as the average turbulent element, we should expect its amplitude of scintillation to lie approximately half-way between that of a star and of a planet like Jupiter whose disk subtends a large angle.

Observations of Mars (4″.4) and Jupiter III (1″.5) showed that the critical angular diameter lay between the two (Fig. 1). We, therefore, take 3″ as the average angular diameter of the turbulent elements, the observations having been made at 30° altitude and with the same telescope aperture. We take the average real size of the turbulent elements to be about 7 cm. Evidence for this is provided by (1) the size of shadows seen on the objective or mirror (2) eclipse ' shadow-bands,' and (3) the fact that maximum brightness scintillation occurs with telescope apertures of about 3 in.

Hence we deduce that the average distance of the turbulent elements is 5·2 km at 30° altitude. This gives a vertical height of 2·6 km.

This estimate is in fair agreement with Rayleigh's value of 4 km, derived from the visual observations of Montigny and Respighi. Minnaert and Houtgast (1935) also found on one occasion turbulent elements located at two levels of 0.6 km and 2·6 km, and moving at different speeds.

Observations of the kind I have described must be carried out with the same telescope aperture. Since the shadows cast on the mirror are of the order of size 7 cm, any increase of aperture above this value includes at one instant a larger number of shadows and the total light shows a smaller variation because of the ' integration effect.' It is also necessary to work at approximately the same altitude because of the amplitude-altitude relation (see, Ellison and Seddon 1952). For these reasons the method is not a very practical one, as a routine procedure, since it involves the use of a 36-inch telescope and the presence at one time of a series of planets of different angular diameters.

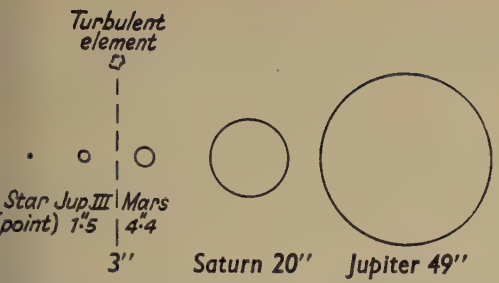

Figure 1. Apparent size of an average turbulent element in relation to the apparent angular diameters of the various planets whose scintillation has been studied.

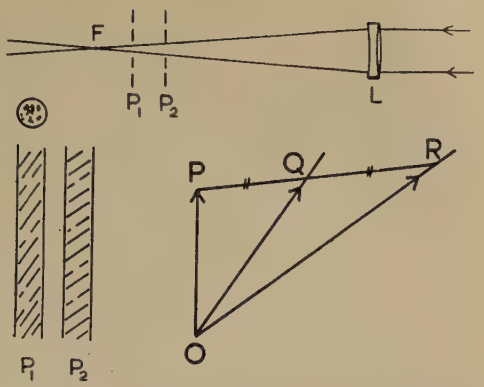

Figure 2. The patterns on the left were obtained by shooting a plate at known speed across the cone of rays at P_1 and P_2 respectively. In the vector diagram; OP is the velocity of the plate; OQ and OR are the relative velocities of the moving shadows in the extra-focal image at P_1 and P_2 respectively; PQ and PR are the actual velocities in the extra-focal pictures P_1 and P_2 (note that PR = 2PQ). The velocity of the shadows across the lens is given by $PQ \times FL/FP_1$ (after Minnaert and Houtgast).

2. Extra-focal image studies

Each turbulent element causes a certain convergence or divergence of part of the light beam. The shadows of these elements move across the lens or mirror; the velocities are relative to the star's motion in the sky. The velocities of the shadows moving across the lens may be measured by shooting a photographic plate at known speed through the out-of-focus image, say at positions 1 cm and 2 cm respectively inside the focus. The resultant track on the plate is crossed by streaks usually having a definite inclination. These streaks give the direction of motion of the shadows in the out-of-focus image relative to the motion of the plate. The shadow velocities across the lens may then be found from a simple graphical construction, if the speed of the plate is known (Fig. 2).

Minnaert and Houtgast, who first used this method (1935), found velocities across the lens on three different nights, of 5·8 m/sec, 5·8 m/sec and 7·0 m/sec. On some nights the results are indeterminate because of indistinct streaks, due perhaps to motions occurring in several different layers moving at different speeds.

3. Colour scintillation

Montigny first made quantitative observations of colour scintillation by the simple expedient of placing in front of the eye-piece of a small telescope a thick plane-parallel plate inclined to the optical axis of the instrument. The plate was caused to rotate about the telescope axis, thereby spreading out the colour sequence on to a circular time-base. The same effect may be seen by focusing a pair of field-glasses on Sirius and vibrating the instrument by hand.

Respighi observed the spectrum of a star at low altitude. He noticed dark bands passing rapidly along the spectrum. On calm nights he found that the bands moved from red to violet for stars in the west and from violet to red for stars in the east. He attributed this phenomenon to the diurnal motion of the earth which carried the turbulent elements across the light beam. In Fig. 4, we see that the violet and red rays, reaching an observer at O, have originated from different parts of the plane wave-front outside the atmosphere. These two rays become more and more separated as we move up through the atmosphere from the point O, and a moving turbulent element affects different colours in turn.

Some years ago I set up an objective prism and a 5-inch lens in order to examine these phenomena. It struck me that a great deal of quantitative information about the motions of the turbulent elements could be derived if the spectrum were broadened on a time-base. This may conveniently be done by observing the spectrum through a rotating square prism of glass, whose axis of rotation is set parallel to the plane of dispersion (Fig. 3).

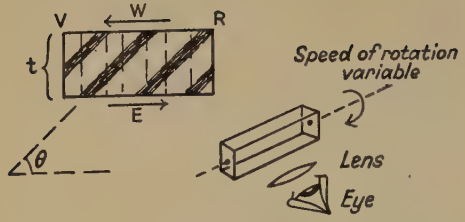

Figure 3. A new type of colour scintillometer. The stellar spectrum is broadened and the time-base supplied by means of the rotating square prism. The inclination (θ) of the dark bands to the plane of dispersion gives a measure of the wave-length interval ($\Delta\lambda$) traversed horizontally by a band in time t.

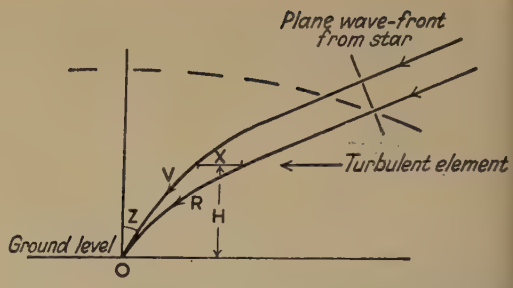

Figure 4. The paths by which two rays of different colours from the same star reach an observer at O. Either through wind drift, or simply by the diurnal motion of the earth, a turbulent element traverses the different spectral colours in succession.

A measure of the angle of inclination (θ) of the moving spectral bands gives the wave-length interval ($\Delta\lambda$) traversed by the turbulent element at height H above the ground in time t (t is variable and is known from the angular speed of rotation of the prism). Let x be the linear separation of the rays V and R whose wavelength separation is $\Delta\lambda$, then $x = f(Z, H)$, Z being the angular zenith distance, and if H is known from one of the other methods x may be found. The velocities of the turbulent elements may be obtained from $u = x/t$. The simplest case is illustrated in Fig. 4, where the velocities are assumed to lie in the E-W plane through the zenith.

Astronomical seeing is usually better on mountain tops, no doubt because much of the turbulence occurs at comparatively low levels. But, as every mountaineer knows, mountains carry up turbulence to great heights. Observations of scintillation should, therefore, be made from aircraft up to 30,000 ft. Useful data might be obtained with a simple device like Montigny's scintillometer, comparing colour effects on Sirius at different heights.

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Interpretation of stellar scintillation

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Stellar scintillation is caused by the inhomogeneity of the refractive index of the moving atmosphere. The changes in refractive index probably arise mainly from convective mixing of air at different temperatures and, to a less important extent as far as light waves are concerned, at different humidities. Pressure changes, resulting directly from the turbulent motion, are probably unimportant except for changes in refractive index on so large a scale (hence occurring so slowly) that their effect would not be recognized as scintillation. Observations are still rather scanty, but for the lower troposphere a temperature fluctuation of about 0·1°C, giving a refractive-index fluctuation of about 2×10^{-7}, appears to be a representative average.

For any detailed understanding, consideration of the spectral distribution of the random fluctuations is essential. Earlier attempts to relate scintillation to periodic variations in the atmosphere have not been fruitful. There are, in a loose sense, predominant frequencies in stellar scintillation, but for quite different reasons.

In general the relationship between the spectrum of temperature fluctuation and that of velocity fluctuation in a turbulent fluid is not at all a simple matter. However, it is reasonable to neglect the effect of the very small temperature changes with which we are concerned on the dynamics of the motion, and to expect the spectrum of temperature fluctuation to be similar in form to that of the velocity fluctuation, provided the length scale of the fluctuations considered is not too large. For air, as Batchelor (1952) has recently pointed out, the smallest length scale, at which molecular diffusion takes control and damps out the fluctuations, is essentially the same for momentum and heat.

The notion of a statistical equilibrium between inertial transfer of kinetic energy from the large eddies through the spectrum, and its dissipation by viscosity in the small eddies, has become familiar in recent years. In the atmosphere the range of eddy sizes, even if still rather ill-defined, is extremely large, corresponding to Reynolds numbers of at least 10^7. In such a case the spectral density of mean-square fluctuation may be expected to vary as the $-5/3$ power of the wave number of a spatial Fourier analysis, for wave numbers between those of the energy-containing and the energy-dissipating eddies. And it is just in this range that the form of the refractive-index spectrum is most likely to be similar to that of the velocity spectrum. Examination of the few available measured fluctuation spectra of stellar scintillation, both for the fluctuation in angle of arrival of starlight (Megaw 1950) and, especially, for the fluctuation in its brightness (Megaw 1953, Fig. 6), strongly suggests that this similarity does indeed exist. For some purposes it is adequate to assume that the action of viscous damping on the small eddies becomes predominant so suddenly as to be represented by an abrupt cut-off of the spectrum at some largest wave number k_s, say.

In trying to explain the facts of stellar scintillation and to use them to learn something about atmospheric turbulence, I assumed that the atmosphere behaves as if it were a homogeneously turbulent stream with a uniform mean velocity, large compared with the turbulent velocities, across the line of sight. For many purposes this over-simplified model takes us quite a long way. The relationship between wave number and fluctuation frequency is then

$$k = \frac{2\pi f}{U}, \text{ where } U \text{ is the mean velocity.}$$

It is important to note that even in shear flow, when the turbulence as a whole is markedly inhomogeneous and anisotropic, these considerations will apply to the smaller eddies. The necessary proviso is that the Reynolds number of the turbulence is very large, as it is likely to be even locally in a stratified atmosphere. This is relevant to the brightness fluctuations of light waves passing through the atmosphere. The fact that for these fluctuations, unlike the angular fluctuations, the smallest eddies (roughly speaking) produce the largest effect is easily understood in terms of ray optics; and if the approximations of ray optics are not valid the statement is no longer true. For a homogeneously turbulent medium the following factors enter as the eddy size under consideration is reduced: (a) more eddies per unit path length ($\propto k$); (b) the transverse gradient of refractive index, for a given 'eddy strength,' increases ($\propto k^2$); (c) the 'eddy strength' decreases ($\propto k^{-5/3}$). The powers of k in brackets represent the corresponding dependence, on wave number, of the spectral density of mean-square brightness fluctuation, and the net result is $\propto k^{+4/3}$; or, if we prefer to think in terms of root-mean-square (r.m.s.) brightness fluctuation, $\propto k^{+2/3}$. This simple result, subject to the uncertainty of the precise relationship between the refractive-index spectrum and the velocity spectrum, is the basic form of the spectrum of stellar-brightness fluctuation. The observed frequency spectrum would have the same form, up to some highest frequency corresponding to the region of viscous damping of the turbulent motion of the air, if the observing aperture were smaller than the size of the smallest eddies (~ 1 cm). With telescope objectives of normal aperture the effect of the small eddies is reduced by the integrating effect of the aperture. At frequencies ($Uk/2\pi$) large compared with that produced by the translation of eddies of size equal to the aperture across the line of sight, the effect of this integration is to reduce the index of the basic spectral power law (of r.m.s. fluctuation) by unity*, i.e., to $f^{-1/3}$ or $k^{-1/3}$. The fact that the observed spectra (Mikesell, Hoag and Hall 1951) do, on at least some occasions, show a region closely corresponding to this $-1/3$ power law (Megaw 1953) is of considerable interest. Discussion of the reason for the more complicated forms which are sometimes observed, and of the significance of the fact that the derived values of the mean velocity U are often less than the expected mean wind velocity, must be deferred to another occasion.

* The integration of course takes place in the space, not the frequency domain; in the former its result is, as might be expected, to *increase* the index of the spectral power law by unity.

Of the measurements of stellar brightness-fluctuation which can be made without facilities for spectral analysis, the dependence on zenith distance (Z), and so on the path length (L) through the turbulent atmosphere, is probably the most illuminating. To order-of-magnitude accuracy the condition of validity for a ray theory treatment of the problem is that the path length should be less than a^2/λ where a is the size of the smallest significant eddies, and λ is the light wavelength. When this is fulfilled, the r.m.s. brightness fluctuation (still for our simple model of a statistically homogeneous atmosphere, but equally valid for a simply stratified model) should be proportional to $L^{3/2}$ or, to a close approximation (sec $Z)^{3/2}$, and independent of λ. This arises simply from the ' $\sqrt{n}$ law ' for the addition of the effect of n statistically independent ' eddies,' and the fact that for small fluctuations the effect of a representative ' eddy ' in increasing or decreasing the instantaneously observed brightness is proportional to its distance.

If, on the other hand, the distance is so large that it exceeds the value of a^2/λ for the *largest* significant eddies, a simple application of wave theory shows that the r.m.s. brightness fluctuation becomes proportional to $L^{\frac{1}{2}}$ and λ^{-1}. The transition from one limiting case to the other can be thought of as the onset of an attenuating effect of diffraction, spreading as the distance is increased from the smallest eddies through the fluctuation spectrum to the largest. Clearly an integration over space as well as over spectral wave number is, in general, involved; and the physics of the matter can easily get submerged in the mathematics. It is only in rather special cases – for instance when the part of the atmosphere near the point of observation contributes little to the observed fluctuation – that the proportionality to $L^{\frac{1}{2}}$ and λ^{-1} might be expected to be closely reproduced in practice.

A comparison of the initial slope of the average observed relationship between brightness fluctuation and sec Z, in logarithmic coordinates, with the predicted form (sec $Z)^{3/2}$ is given in Fig. 1. The agreement shown here appears to be general for telescope aperture greater than about 6 in., and Z less than about 60° (see also the observations of Nettelblad 1953). The interpretation of the fact that over a range of larger Z the slope is near (sec $Z)^{\frac{1}{2}}$ is open to a good deal more doubt, though we may note provisionally that this is another respect in which the atmosphere behaves approximately *as if* our simple initial assumptions were true. The trend towards independence of Z as $Z \to 90°$ could have several causes, of which the failure of the approximation $L \simeq H$ sec Z is probably not a significant one here. What is certainly significant, among other factors such as the possible reduction of the mean cross-wind, is that the fluctuations

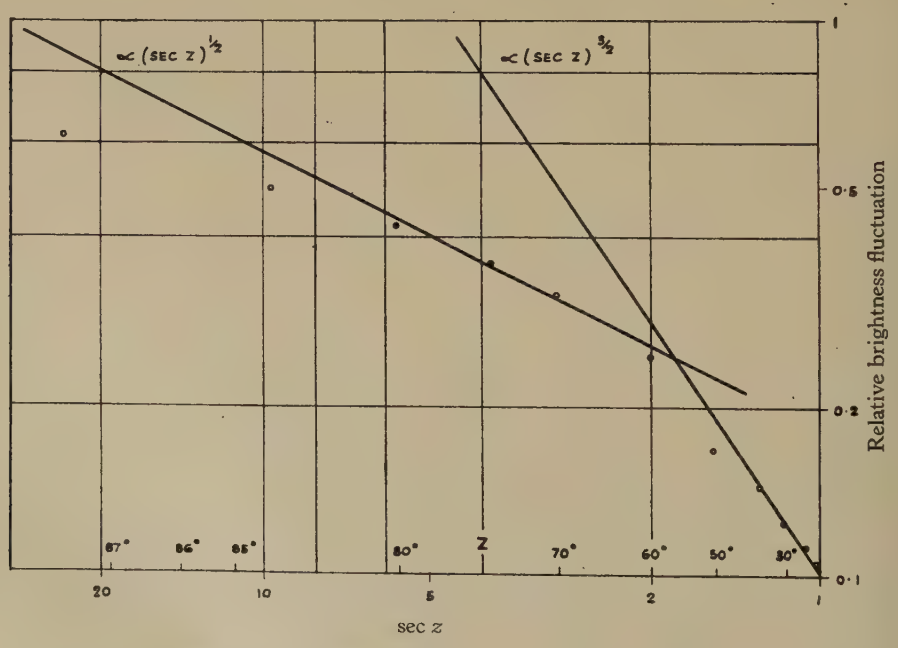

Figure 1. Comparison of observed and predicted dependence of r.m.s. brightness fluctuation on zenith distance (Z). Circles represent observations of H. E. Butler (1952, Fig. 7) for 15 in. aperture. The ordinate scale is relative r.m.s. brightness fluctuation in natural logarithmic units, on which unity = 1·09 mag.

are by no means small for stars near the horizon; and that in the limit of large path length something like a Rayleigh distribution of brightness fluctuation, independent of L, must be expected. (In replotting Butler's observations a log-normal distribution has been assumed, with some support from his Fig. 8, for all values of Z; a closer study of the actual probability distribution by measurement of the appropriate integral moments would be of much interest.)

The implication of the restriction as regards the 3/2 power law, in the preceding paragraph, to not-too-small telescope aperture seems clear: even near the zenith, ray theory is not quite valid for the smallest and most distant eddies; but the contribution from these to the observed total brightness fluctuation is made negligible by the aperture integration of all but the smallest astronomical telescopes.

Much has had to be left out of this introductory discussion and several intriguing problems remain. We see, however, that there are good grounds for believing, as was tentatively concluded in Megaw (1950), that the facts of stellar scintillation can be explained and unified as a manifestation of a universal equilibrium spectrum of atmospheric turbulence. Of the problems, one of the most intriguing is to understand in detail how this comes about in an atmosphere which is far from being the homogeneously turbulent and uniformly flowing stream from which we started out.

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The analysis of stellar scintillation phenomena

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1. Introduction

When we look at the night sky, we see at once that the brightness of the stars changes quickly, but, when we look through the eyepiece of a telescope, the phenomenon appears differently: the twinkling is less obvious but the stars seem to move and the image is somewhat blurred. Nevertheless it is the same phenomenon but the point of view of the astronomer is different from that of the ordinary man which is also the point of view of the meteorologist. The first is interested in what may be called phase-scintillation and the second with the intensity scintillation.

Let us consider a beam of light reaching the ground; it may be represented by a monochromatic wave: $\psi \exp i\,(cnt + \phi)$. If scintillation occurs, we observe a perturbed wave: $\psi'\,(t) \exp i\,(cnt + \phi')$ (ψ, ψ', ϕ and ϕ' being real numbers). We may then define the phase-scintillation as the standard deviation of ϕ:

$$\sigma_\phi{}^2 = \overline{(\phi - \phi')^2} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and the intensity-scintillation by:

$$\sigma_I{}^2 = \overline{(\psi^2 - \psi'^2)^2} \quad . \quad . \quad . \quad . \quad (2)$$

The first quantity cannot be observed by a single observer because ψ is not known but two separate observers can measure it. If the observers are sufficiently far apart to ensure independence of scintillation (a distance of some metres seems to be adequate), we have:

$$\overline{[(\phi - \phi_1') - (\phi - \phi_2')]^2} = \overline{(\phi_1' - \phi_2')^2} = 2\sigma_\phi{}^2$$

The Michelson interferometer is a suitable instrument for this measurement. If observers are close together, for instance at two points of the same telescope mirror, the difference of phase is less than in the former case but it explains the apparent displacements of stars; we have

$$\alpha = \frac{\phi'_1 - \phi'_2}{nx} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

α being the apparent angular displacement of the star and x the distance.

2. Scintillation mechanism

It is now well established that scintillation is due to local fluctuations of refractive index in the atmosphere, these fluctuations being due themselves to turbulent variations of temperature. This is the reason why scintillation is important for meteorologists.

As we shall see later, the intensity-scintillation is mainly due to high tropospheric or low stratospheric layers but the lower part of the atmosphere plays a prominent rôle for phase-scintillation.

Theoretically, the problem reduces to the study of the propagation of light through an inhomogeneous medium and this can be done in two ways: by a ray-treatment or geometrical optics, and by a wave-treatment. The former is certainly easier but the latter is more rigorous.

In any case the treatment must be statistical, indeed we do not know the actual distribution of temperatures in the atmosphere nor the actual distribution of light at the ground, but we can measure or compute the statistical properties of these fields.

3. Statistical representation and method

The most important of these statistical quantities are the standard deviation, the covariance and the power-spectrum. They are defined, in the case of the refractive index, as follows:

$$\sigma_\nu^2 = \overline{(\nu - \bar{\nu})^2} \equiv \overline{\nu^{*2}}, \quad \nu = \text{refractive index}$$

$$\mathrm{cov}_\nu(\xi, \eta, \zeta) = \overline{\nu^*(x, y, z)\, \nu^*(x + \xi, y + \eta, z + \zeta)}$$

$$S_\nu(k, l, m) = \frac{1}{(2\pi)^{3/2}} \int_{-\infty}^{+\infty}\int\int \mathrm{cov}_\nu(\xi, \eta, \zeta) \exp i(k\xi + l\eta + m\zeta)\, d\xi\, d\eta\, d\zeta$$

We must therefore establish relations between these quantities and the corresponding statistical characteristics of the intensity and phase fields. This has not yet been done completely. Following T. H. Ellison (1951), several authors have solved the problem to a first linear approximation.

The problem may be solved quite generally at the cost of some mathematical intricacies. I have been lucky enough to show that the problem may be solved by a partial differential equation of the second order in the geometrical theory. The wave treatment leads to an integro-differential equation which does not permit much hope of practical results, but if we assume that the intensity scintillation is due to a rather thin layer (about 1 km thick), a solution may be found as an integral expression.

The linear geometrical approximation is always valid for the phase-scintillation.

4. Some results

I shall not go into mathematical developments and I shall give the final results in the simplest case of linear approximation.

Let us take first the case of a turbulent layer of thickness ϵ, at an altitude z above the ground, for a star at the zenith. Let us take:

$$k_0^2 \equiv \frac{\iint S_\nu(k, l, o)\,(k^2 + l^2)\, dk\, dl}{\iint S_\nu(k, l, o)\, dk\, dl}, \quad . \quad . \quad . \quad (5)$$

k_0 being a mean horizontal wave-number of the refractive index. $l_0 = 2\pi/k_0$ will then be the mean horizontal wave-length of the same field.

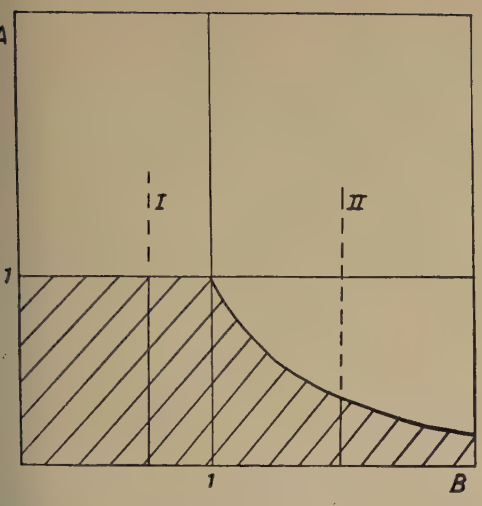

Figure 1. Validity of geometrical linear approximation. The vertical lines I and II correspond to weak and strong scintillation.

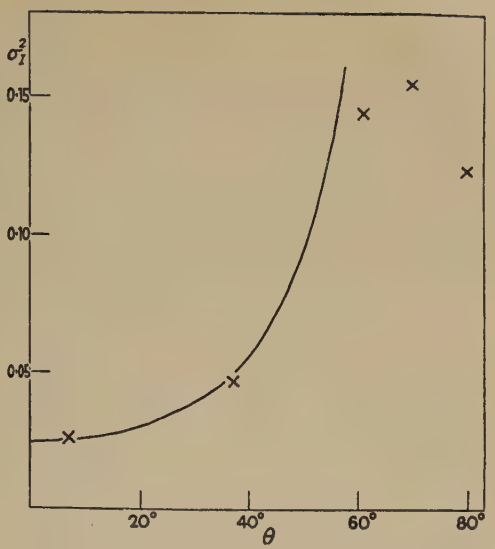

Figure 2. $\sec^3\theta$ law. A set of five observations taken during the same night is compared with the theoretical curve.

A simple formula relates the standard deviations of refractive index and temperature.

$$\sigma_\nu = \frac{6{\cdot}8 \times 10^{-5}\, p}{T^2}\,\sigma_T \simeq 4 \times 10^{-7}\,\sigma_T \text{ (at the 10-km level)} \quad . \quad . \quad (6)$$

Let us now put :

$$V = \frac{1}{2\pi}\int\int S_\nu(k, l, o)\, dk\, dl \simeq \frac{\sigma_\nu^2}{k_0} \quad . \quad . \quad . \quad (7)$$

We have then :

$$\sigma_\phi^2 = \epsilon\, V n^2 \equiv A, \quad . \quad . \quad . \quad . \quad (8)$$

$$\sigma_\alpha^2 = k_0^2\, \epsilon\, V. \quad . \quad . \quad . \quad . \quad (9)$$

These quantities may be observed, the last one being the radius of the photographic image f the star.

We have for the intensity spectrum :

$$S_I(k, l) = (k^2 + l^2)\, \epsilon\, z^2\, S_\nu(k, l, o) \quad . \quad . \quad . \quad (10)$$

nd

$$\sigma_I^2 = \epsilon\, V z^2 k_0^2 k_m^2. \quad . \quad . \quad . \quad (11)$$

here k_m corresponds roughly to the maximum of the intensity spectrum curve.

The geometrical approximation is now valid for intensity computations when,

$$B \equiv k\, k_0\, z/n < 1 \quad . \quad . \quad . \quad . \quad (12)$$

d cannot therefore be used for the whole range of k, but is generally verified for almost the hole of the significant range.

The linear approximation is then valid when,

$$A B^2 = \epsilon\, V k^2 k_0^2 z^2 \ll 1, \quad . \quad . \quad . \quad (13)$$

d, comparing with (11) we see that, near the maximum of the spectrum curve, this quantity equal to σ_I^2 which is small for stars near the zenith (cf. Fig. 1).

For stars of zenithal distance θ the quantities ϵ and z must simply be multiplied by $\sec\theta$ d therefore σ_I^2 and S_I are proportional to $\sec^3\theta$. This relation constitutes a test for the theory.

When we consider the whole atmosphere and not a single layer, we see that the formulae 0) and (11) can still be used, with a reasonable accuracy, if z is taken as an effective altitude about 10 km and the thickness ϵ may range between 1 and 3 km.

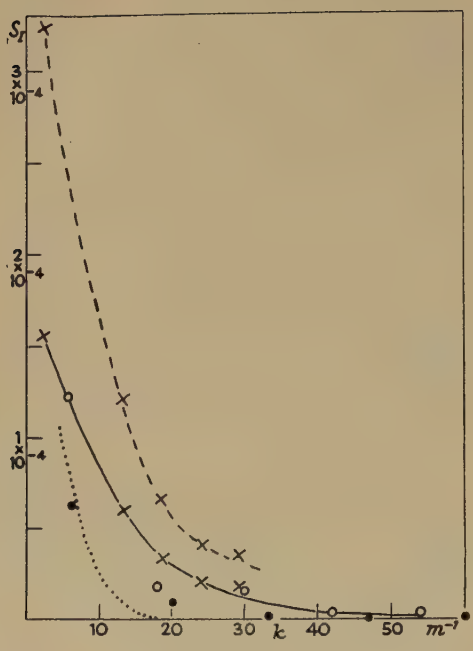

Figure 3. Spatial intensity spectra. The upper curve is the spectrum of a star at 37° zenithal distance. The middle one corresponds to a star at the zenith. The last one is a theoretical curve, deduced from the preceding curve by taking account of the rather large telescope aperture (15 in.). Crosses, circles and dots are experimental points corresponding to the three curves but the crosses near the second curve are experimental points for the 37° star, corrected to zero zenithal distance according to the $\sec^3\theta$ law.

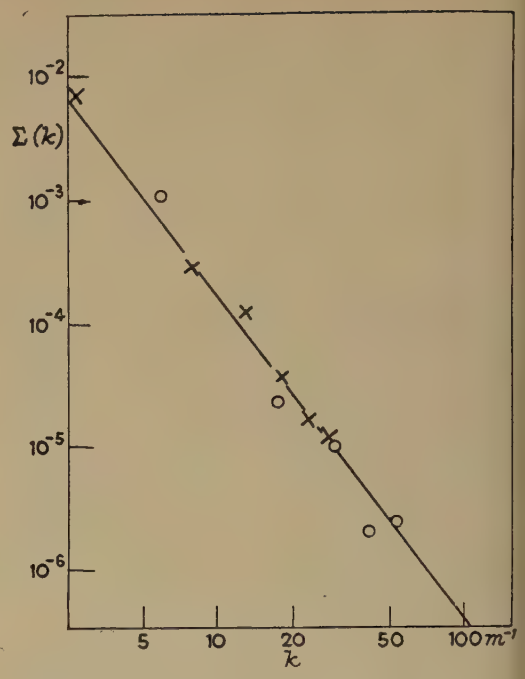

Figure 4. Spectral distribution of thermal fluctuations in the atmosphere deduced from scintillation. Σ is the three-dimensional spectrum of thermal fluctuations integrated on angular parameters. The power law giving the best fit corresponds to $\Sigma \propto k^{-8/3}$ (the crosses and circles have the same significance as in Fig. 3).

On the other hand, formulae (8) and (9) for the phase-scintillation are different and we must write :

$$\sigma_\phi^2 = z\,\overline{V}\,n^2,$$

$$\sigma_\alpha^2 = k_0^2\,\overline{V}\,z$$

where $\overline{V}$ is some weighted mean value of V and z an effective thickness of the turbulent atmosphere which may be taken as the height of the tropopause.

5. Example of scintillation analysis

The analysis of scintillation records obtained by Butler at Dunsink Observatory is summarized in Figs. 2, 3 and 4. The last one shows that in the spectral range where computations can be made, owing to the length of the records, the spectrum of thermal fluctuations in the atmosphere follows roughly a law of the form k^{-3} and the amplitude of these fluctuations of the order of 0·01°C.

Acknowledgments

I desire to express my special thanks to Professor P. A. Sheppard for having first suggested to me the investigation of this problem and for his constant encouragement during the execution of the work. I wish also to express my thanks to the British Council for a maintenance grant.

References

Ellison, T. H. 1951 *J. Atmos. Terr. Phys.*, **2**, p. 14.
Isacker, J. Van 1953 *Publ. Inst. R. Mét. Belg.* B, **8**.

Discussion

Mr. J. A. Ratcliffe : I should like to draw the attention of the meeting to some of the important ideas about stellar scintillations which have been emphasized recently by those engaged in the study of Radio Stars (see References).

First : it cannot necessarily be assumed that the mean extent of the irregular amplitude or phase variations of the light observed at the ground is of the same order as the size of the irregularities of refractive index causing them. It may be much smaller, and the two are only of the same order if during the scintillations the observed amplitude does not approach too near zero.

The second point is that, in judging whether the observed fluctuations send the amplitude near to zero, it is important to know how many independent irregularities occupy the aperture of the observing telescope. If there are more than one then the resulting integration will lead to the impression that the amplitude does not fluctuate to zero, even if, in fact, it does so in one single irregularity.

The third point is that the auto correlation function $\rho(\xi)$ of the complex light-amplitude at the ground is closely related to the same quantity computed for the phase of the wave just after it has passed through the phase-changing atmosphere : indeed for an infinitely small star and for irregularities of refractive index confined to a plane the two quantities are equal. Now it can be shown that $\rho(\xi)$ is equal to the mean value of the visibility of the interference fringes observed with a Michelson stellar interferometer having its mirrors a distance ξ apart. Useful results could be obtained by measuring this visibility for a series of different distances.

References

Booker, H. G., Ratcliffe, J. A. and Shinn, D. H. 1950 'Diffraction from an irregular screen with applications to ionospheric problems,' *Phil. Trans.* A, **242**, p. 579.

Fejer, J. A. 1953 'The diffraction of waves in passing through an irregular refracting medium,' *Proc. Roy. Soc.* A, **214**, p. 455.

Hewish, A. 1951 'The diffraction of radio waves in passing through a phase-changing ionosphere,' *Proc. Roy. Soc.* A, **209**, p. 81.

1952 'The diffraction of galactic radio waves as a method of investigating the irregular structure of the ionosphere,' *Proc. Roy. Soc.* A, **220**, p. 494.

Mr. E. Gold : If scintillation is due to eddies in the path of the light, I should expect the operative eddy to be now at one level, now at another, the changes occurring at short intervals of time – a second or two. If this is so, then the winds determined from the observations of scintillation ought to vary appreciably. If they do not, in fact, so vary, the deduction could be that the operative eddies on an individual occasion are more or less at one level. May I ask Dr. Ellison or Dr. Butler if the observations of wind mentioned do indicate variations ?

Dr. P. B. Fellgett : Astronomers cannot help implicitly observing atmospheric disturbances, since these constitute one of the most formidable hindrances to their work. Unfortunate as this circumstance is for purely astronomical purposes, it does imply that astronomical methods may be particularly suited to meteorological studies of turbulence in the atmosphere.

In order to design and interpret experiments on scintillation, it is necessary to understand three processes : the way in which the wave-fronts of the starlight become disturbed in the turbulent layer or layers; the propagation of the disturbances as the wave progresses; and the effect on the final image of disturbances which arrive at the telescope objective.

Taking these topics in reverse order, the evaluation of the effect on the properties of the telescopic image seems to present no essential difficulty; a solution for the effect on the brightness has just been given by Dr. Megaw.

The second problem, that of propagation of the disturbances, was treated in a statistical manner, which is the way in which it is significant in scintillation since no importance can be attached to an individual twinkle, by Ratcliffe (1948, Nature **162**, p. 9), with reference to the formally similar problem of fading of radio signals caused by irregularities in the ionosphere. It was developed for this purpose by Briggs, Phillips and Shinn (1950, *Proc. Phys. Soc.* B, **63**, p. 106), applied to scintillation of radio stars by Ryle and Hewish (1950, *M.N.R. Ast. Soc.*, **110**, p. 381) and presented in detail, without restriction to radio phenomena, by Booker, Ratcliffe and Shinn (1950, *Phil. Trans.* A, **242**, p. 579). This theory was applied to the circumstances of scintillation of starlight by Little (1951, *M.N.R. Ast. Soc.*, **111**, p. 289). It appears entirely adequate to cover

the propagation of disturbances in the wave-front, and the situation with regard to this part of the problem seems particularly satisfactory in view of the fact that the behaviour is not only described in this theory in terms that admit a ready physical interpretation, but which also accord well with the language that has become familiar in studies of turbulence; in particular, the phenomena are characterised by their auto-and cross-correlation functions and the corresponding ' power spectra.'

With regard to the first of the three processes, it is hardly doubted that the cause of disturbance to the wave-fronts is irregular refraction. The change of refractive index in a wavelength may always be assumed small. If the irregularities occur in layers sufficiently thin for the deviation of the energy while inside the layer to be small compared with the micro-scale of the disturbance, the treatment is very simple. Although different opinions have been expressed by the various speakers, I understand that it is quite possible meteorologically that the turbulence with which we are concerned in scintillation occurs primarily in thin layers, and the astronomical observations certainly seem to suggest that this is often true. If so, it should readily be possible to obtain, under suitable conditions, the time- and space-spectra of the turbulence, and the wind velocities in the turbulent layer, by the method of Briggs, Phillips and Shinn (1950); that is, by measuring the auto- and cross-correlation functions of the brightness variations of the same star observed with telescopes of a few cm. aperture suitably spaced in the plane of the wave-fronts. Similar observations of different stars in the field of a single telescope would yield the angular velocity of the layer or layers, and comparison with the linear velocity then gives at once their heights. Would meteorologists be interested in such data ?

Thick layers present more difficulty, and Dr. van Isacker has told us about some of the possible approaches. The irregularities which the wave meets in the lower part of such a layer depend on the deviation it suffered higher up ; this is often expressed as " multiple diffraction." Mr. Ratcliffe has already mentioned the work of Hewish (1951, *Proc. Roy. Soc.*, A, **209**, p. 81) which bears on this problem, and the paper by Chandrasekhar (1952, *M.N.R. Ast. Soc.*, **112**, p. 475) is in the list of references which is before this meeting. I would like to draw attention also to the discussion of Keller (1953, *Astronom. J.*, **58**, No. 1209), and to that of Bhatia and Noble (1953, *Proc. Roy. Soc.*, A, **220**, p. 356; it is explicitly concerned with diffraction by ultrasonics)*. The mathematics in these papers is heavy, and this makes it especially tempting to simplify by using, as Chandrasekhar has done, the geometrical theory which is necessarily included in any proper wave theory as the limiting case for radiation of very short wavelengths. However, radiation diffracted into different directions corresponds formally to sidebands of different frequency in phase-modulation of a sinusoidal carrier (cf. Hewish 1951 and Hund 1942, *Frequency Modulation*, McGraw-Hill, esp. pp. 29-37), and from the known behaviour of such sidebands we can see that the total phase excursion, whenever this is small, determines the intensity of the diffracted beam, while the direction then depends only on the wavelengths of the irregularities. Ray theory predicts that all the radiation is always deviated, and that the direction depends on the phase excursion; this is not even an approximation to the facts of diffraction, but is qualitatively different. Similarly, as Ratcliffe and Little have emphasized, interference between the direct and the deviated beams (the latter are delayed because of their oblique direction of travel) is often the primary cause of brightness scintillation, and geometrical theories ignore it. I would urge, therefore, that the geometrical approximation should be used only with the greatest caution; I am doubtful if it can be safely used even to decide whether a layer can be regarded as thin. It admittedly becomes more nearly valid as the total phase excursion becomes very large, but by the time these conditions are encountered the astronomer (at least) has gone home to bed.

Mr. L. C. W. Bonacina : Is it general experience that scintillation is more conspicuous in polar than in tropical air ? I should say myself it is, but am not sure, and in any case there must be many exceptions. I would also ask for a few details concerning the colour-scintillation analysis of Sirius. To my eyes, and presumably to most normal eyes, that star flashes blue, but I have met persons who say that they frequently see it look red.

We should appreciate the fact that upon this very phenomenon of stellar scintillation or twinkling the beauty of a nocturnal landscape largely depends. The sight of a bright star winking above the crest-line of a range of hills inspires us, but would it do so if the light were steady ? I doubt it.

In Wordsworth's Epilogue there is a fine reference to stars which seem,

> Humbly to hang, like twinkling winter lamps,
> Among the branches of the leafless trees.

* An important paper (J. A. Fejer, 1953, *Proc. Roy. Soc.* A, **220**, p. 455) has since appeared.

Mr. D. R. Grant : I should like to show some records of temperature fluctuations measured by aircraft of the Meteorological Research Flight. The first example (Fig. 1) shows warm and cold patches of air of a type present at all levels and at all times in the troposphere and lower stratosphere. The temperature varies by about 2°F over distances of the order of 10 miles.

The second example (Fig. 2) shows some records obtained using a thermometer with very low lag coefficient. Fig. 2 (a) shows the most common type of fluctuation obtained. It is seen that the ' peak-to-peak ' amplitude is less than 1°F and the scale is of the order of 50 ft. This type of fluctuation, although usually smaller in amplitude, has been obtained in every flight using this special thermometer. It is most common at levels at which inversions of temperature are present. The amplitude of temperature fluctuations appears to decrease as the scale decreases and it would appear that the temperature fluctuations on a scale of 10 cm would be extremely small, perhaps too small to explain scintillation. It may be possible that scintillation is caused by many layers of large-scale turbulence of the type illustrated instead of the more usual explanation of one or two layers of very small-scale ($\sim$ 10 cm) turbulence.

Fig. 2 (c) shows an example of patches of warm and cold air which are present on occasions in the atmosphere. These are much rarer than the first type of record.

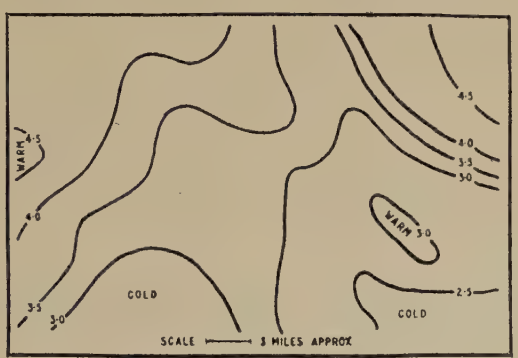

Figure 1. Isotherms (°F) in 520-mb surface.

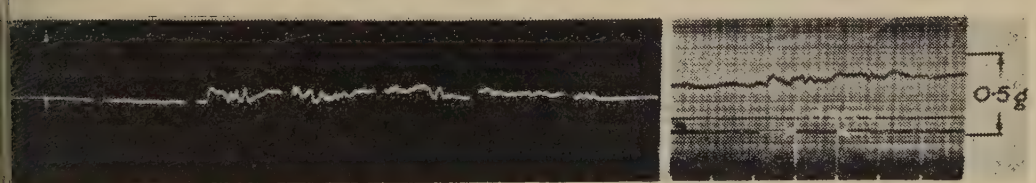

Figure 2 (a). Fluctuations in the horizontal, starting very suddenly at a height of 17,000 ft.

2 (b). Accelerometer record during same period.

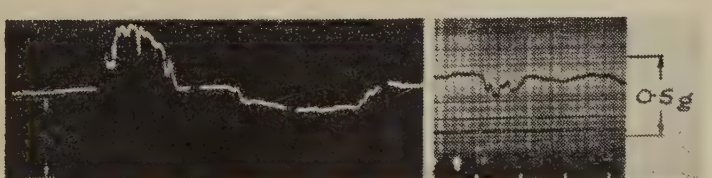

Figure 2 (c). Example of large 'pulses' of warm and cold air at a height of 17,000 ft.

2 (d). Accelerometer record during same period.

In all the above photographs of the ultra-rapid thermometer traces there are gaps at 3-second intervals (approx. 900 ft); temperature increases upwards from the lower edges of the illustrations. In the accelerometer traces, Figs. 2 (b) and 2 (d), an upward acceleration of the aircraft is shown by a deflection of the trace downwards and the 3-second time marks are shown at the lower edges of the illustrations.

Dr. R. S. Scorer : It is reasonable to suppose that radiation from haze tops or tops of moist layers produces convective stirring under small inversions, particularly at night. Such stirring has nothing to do with velocity changes that may be found at inversions for both the velocity change and the inversion are the result of the convection below. Austin (1952) reported cloud elements about 2-3 ft across and it seems likely that such convection in clear air would have similar dimensions, if that was the order of the thickness of the over-turning layer. An aircraft would only encounter such layers fortuitously because they need not have great horizontal extent. In such layers velocity and temperature fluctuations would be *closely* associated. The corrugation of the inversion might also cause scintillation.

Thin layers of cobblestone turbulence, which I have suggested are the result of shear generated in waves over hills at levels where the lapse rate is discontinuous, have been met by glider pilots; but because the motion is not produced by density differences, but results from very large shear, temperature variations are probably less pronounced than in the case first suggested.

The ' boiling ' of the sun is more probably associated with convection from the ground. Such convection is often visible to the eye and was long ago remarked on by Hankin (Scorer 1954), for instance.

In view of this, any agreement with deductions from the assumption that the motion is homogeneous isotropic turbulence (supplied with energy from the mean flow and damped by viscosity) is something to be explained rather than an explanation.

References

Austin, A. R. I.	1952	*Weather*, **7**, No. 2, p. 50.
Scorer, R. S.	1954	*Quart. J. R. Met. Soc.*, **80**, p. 68.

Mr. Newstead : One of the most significant things about the scintillation of radio stars is the diurnal variation and I should like to ask Drs. Butler and Ellison if there is any such effect in optical scintillation.

Has Dr. Ellison made a statistical analysis of the amplitude distribution of his fluctuations ? In the case of radio stars the distribution is a displaced Gaussian one for weak amplitudes tending to a Rayleigh distribution for large amplitudes (cf. Hewish 1952, *Proc. Roy. Soc.* A, **214**, p. 492, Fig. 6).

Has Dr. Ellison considered whether the ' boiling ' visible with a low-powered telescope on the sun's edge would be a useful tool for meteorologists to use to determine wind directions in the upper atmosphere ?

Dr. M. A. Ellison (*in reply*) : Mr. Ratcliffe has raised some important points. In regard to his first : our photoelectric records of stellar scintillation (Ellison and Seddon 1952) cover all apertures from 36 in. down to 3 in. The amplitudes of fluctuation increase approximately in inverse ratio to the receiving aperture of the telescope. This is what we call the " integration effect," and it appears to indicate that the ground ' shadows ' of the turbulent elements are of the order of size 3 in. rather than 36 in. In reply to his second point : even with the smallest aperture used (3 in.), the amplitudes never fall below about 30 per cent of the mean level, notwithstanding the fact that we then obtain short peaks (of about one-hundredth second duration) which may rise to 400 per cent of mean level. I think we are justified in assuming, therefore, that the ground shadows, for visible wavelengths, are not far removed in size from that of the refracting irregularities which cause them.

In reply to Mr. Gold, I believe that on many nights, I don't say all, the refracting irregularities can be located at a perfectly definite level, in the height range 2-4 km.

In reply to Mr. Bonacina, when we spread out the colours of Sirius on a time base we see predominantly blue, green and violet, but occasionally yellows and reds. This is what we should expect from a B-type star, whose light is most intense in the blue end of the spectrum.

In reply to Mr. Newstead, there are no doubt big differences in stellar scintillation between night and day, but we cannot yet observe the latter.

For the statistical analysis of the amplitude fluctuations, we have many hundred feet of film available to anyone who cares to investigate the problem !

As to using the movements of the turbulent elements to estimate upper atmosphere wind directions, this can be done with considerable accuracy. In the daytime one observes the irregularities moving along the limb of the sun, at night the limb of a planet like Jupiter is suitable. With a position-angle micrometer I have been able to set upon the wind direction with an accuracy of 10°. One must also know the height, if the observations are to be of value to meteorologists.

Dr. H. E. Butler (*in reply*) : In reply to Mr. Gold, there is definite evidence that the ' wave patterns ' on telescope mirrors, of which we have been speaking, are associated with winds at different heights. It is sometimes possible to detect in this way two or more such apparent wave patterns caused by winds in different directions. Both Gaviola (1949, *Astronom. J.*, **54**, p. 155), and Mikesell, Hoag and Hall 1951 (*J. Opt. Soc. Am.*, **41**, p. 689) have published photographs showing this effect. In fact, one of my slides was taken from the latters' work and illustrated this point.

In reply to Mr. Bonacina, I have seen just one reference to the fact that the amplitude of visual twinkling may be very small *in polar regions*. The observer actually recommended, as a result of his observation, that astronomical observatories (1948, Dietz, *Popular Astronomy*, **56**, p. 202) should be sited in polar regions. I have no information at all which relates scintillation to the source of the air mass actually overhead.

Dr. E. C. S. Megaw (*in reply, communicated*) : Mr. Ratcliffe has drawn attention to several important points which have emerged from studies at the Cavendish Laboratory related to the scintillation of radio stars. His first point might be put more generally by saying that when the observed light fluctuations are not small their relationship to the spatial fluctuations of refractive index from which they arise becomes much less simple. This is one reason for the note of caution sounded in my remarks on the interpretation of the observed characteristics of the scintillation of stars far from the zenith. However, the fact that the spatial scale of the intensity fluctuations of starlight is much smaller than that of the refractive index fluctuations in the troposphere is not a consequence of this limitation. It is equally true for the scintillation of terrestrial lights observed at quite short distances. (' Scale ' here signifies some simple mean value such as the infinite integral of a correlation function). This is one of several aspects of the problem which are more easily understood in terms of (fluctuation) spectral distributions than of correlation functions or overall mean values. Mr. Ratcliffe's second point, relating to one of the effects of aperture integration, reminds us of another such aspect. The effect of a change in telescope aperture depends not only on the number of ' irregularities ' included but also on the spectral distribution, measured or assumed, over the appropriate interval of the length scale.

Dr. Fellgett's contribution has ranged over a number of interesting topics. In spite of the fact that different facets of the subject have been emphasized by the considerable number of workers to whom he refers, and the fact that the theoretical and the experimental conclusions have not been often brought together in the same paper, I think the picture which emerges is already a good deal clearer than his remarks might suggest. The limitations of ray theory are clearly definable; and where they are exceeded a sufficiently approximate application of wave theory for the astronomer's purposes is not difficult, at any rate within the bounds of first-order perturbation theory. The real theoretical difficulty lies elsewhere, namely in analytical manipulation of expressions which represent at all closely the spatial distributions of refractive index which appear to exist in nature. I find the latter part of Dr. Fellgett's remarks on the limitations of ray theory puzzling, though I may have misunderstood what was intended. I certainly do not agree with the belief, attributed to Ratcliffe and Little, that ' interference between the direct and the deviated beams . . . . . is often the primary cause of brightness scintillation.' This would appear to be a revival of Arago's theory of scintillation, long ago disposed of by Rayleigh (1893, *Phil. Mag.*, **36**, p. 129).

I have every sympathy with Dr. Scorer's protest at my over-simplification of the meteorologist's atmosphere. But I still think it was a better starting point than the single-layer idealisation of some other discussions of the problem. Both theory and recent measurements leave no doubt that there are fluctuations of refractive index throughout the troposphere (and presumably beyond) all the time. Both optical and radio measurements of the fluctuations and scattering of radiation passing through the troposphere show a surprising degree of statistical uniformity which would not readily accord with a few-layered model, but would certainly not exclude an ever-changing multi-layered one. Indeed the most recent radio measurements of refractive index distribution suggest that the latter is near the truth; and the consistent form of the extreme high-frequency end of measured spectra of stellar brightness fluctuation points in the same direction. As a speculation, however, I would guess that the broad characteristics of stellar scintillation would not be greatly changed if *either* the ' layers,' *or* the more nearly homogeneous atmosphere in between, were entirely removed. It follows from these views that I do not think experiments designed to measure the height of ' the ' layer would be very profitable, though they would sometimes yield unambiguous results as has, in fact, already been reported in the literature.

Dr. J. Van Isacker, (*in reply*) : I agree with the remarks made by Mr. J. A. Ratcliffe but some comments seem to be necessary. The mean extent of the phase variations is generally

of the same order of magnitude as that of the mean extent of the irregularities of the refractive index aloft, but the mean extent of the intensity variations is always smaller. This is due to the k^4 factor in the formula relating the two spectra.

This essential difference between phase and intensity scintillations must always be taken into account, for instance when the thin-layer hypothesis is introduced. As Dr. R. S. Scorer has pointed out, there may exist some thin turbulent layers in the lowest part of the atmosphere; these layers will then cause intense phase-scintillation but no intensity-scintillation.

In any case, all the troposphere and possibly a part of the stratosphere are turbulent more or less and play a role in phase-scintillation. The top of the troposphere and the lowest part of the stratosphere will only produce intensity-scintillation because of the marked altitude-density effect; this is the only way in which we may use the thin-layer hypothesis as a rough approximation.

The hypothesis of isotropy of turbulence may be justified in the intensity-scintillation theory because the big anisotropic eddies which contain the major part of the energy and which are effective for heat and momentum transfer may be disregarded in intensity-scintillation study. The important eddies with regard to scintillation are those small and isotropic eddies created by a decay process from the biggest ones.

The study of scintillation may certainly be made with the methods used for other wave-propagation phenomena as Dr. P. B. Fellgett suggested, but we must pay attention to the particular nature of each problem. A straightforward application of formulae and methods without careful examination of the hypothesis on which they are based is extremely dangerous. On the other hand, when ray-treatment is valid, i.e., when certain relations between the wave-length of light, the mean size of eddies and the altitude of turbulent layers are satisfied, there is no qualitative difference between ray and wave-treatments; there is only a difference in description.

551.510.42 : 551.575

Smog

As reported on p. 279 of this volume, a discussion meeting was held on 20 January under the above title, with special reference to the 1952 disaster. The opening papers and general discussion are given below.

551.575

Meteorological aspects of smog

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1. Introduction

The interplay of meteorological conditions and atmospheric pollution formed the subject of the Presidential Address to this Society six years ago (Dobson 1948). The purpose of the present contribution is to give some general descriptive information on fog, with more detail for the visitation of 5-9 December 1952, as background to the discussion on other aspects of smog.

2. Fog and smog – general

We are here concerned mainly with fog which forms in quiet meteorological conditions, e.g., in the central part of an anticyclone. When there is little or no cloud in the evening or at night the surface of the ground is markedly cooled by outward radiation of heat. The air in contact with the ground is also cooled. If there is little wind the air nearest the ground becomes colder than the air at some distance above: there is an inversion or reversal of the characteristic daytime decrease of temperature with height. In the inversion layer the mixing processes, which operate between the surface and upper air layers in conditions of brisk air motion and decrease of temperature with height, are greatly reduced or non-existent : the layer stagnates and its relative humidity increases. If, as cooling proceeds, the air temperature in the surface layer falls to the saturation point, water-droplet fog is formed and gradually increases in vertical depth. The surface inversion layer may become several hundreds of feet deep. Outward radiation from the fog top assists the upward growth of the fog. In the general conditions mentioned the slow drainage or drift of cooled surface air from higher to lower ground promotes the development of fog in hollows and valleys.

The atmospheric conditions which lead to the formation of fog also result in suspended impurities, gaseous or solid, being trapped within the inversion layer. Pollution produced in this layer is not dispersed by upward diffusion to the atmosphere above the layer, cleaner air from above is not brought downwards by turbulence, and removal of pollution by horizontal transport is slow and uncertain. High concentrations of impurities then exist in intimate association with the water droplets of the fog: the unpleasant and harmful phenomenon known as ' smog.' The larger number of minute hygroscopic nuclei (products of combustion) in polluted air as compared with clean air is likely to result in a greater number of very small fog droplets. This and the general dirtiness of a polluted fog probably explain the tendency for such fog to persist throughout the daylight hours, especially in the mid-winter months. Even in the absence of water fog, smoke accumulation in the surface inversion layer – perhaps capped by a cloud sheet – can give rise to darkness and gloom.

An indication of the effect of pollution on the transparency of the atmosphere is the fact (Corby 1951) that at Northolt Airport in winter months fog (visibility less than 1,100 yd) is about ten times more frequent in easterly winds, from the bulk of Greater London, than in westerly winds. Wright (1939) concluded that at Kew Observatory smoke particles are the dominant cause of low visibilities and estimated that in average afternoon conditions at that place a visibility of 50-200 yd would become 1 to 2 mi if the smoke particles were removed.

The effect of polluted atmosphere in reducing the duration of bright sunshine is strikingly shown in Fig. 99 of Bilham (1938) and Fig. 20 of Brooks (1950). In January and December the duration of sunshine recorded in Central London is only 45 per cent of that in surrounding country districts. The total loss in the months November, December and January is more than 90 hr and for the whole year approaches 300 hr. The loss doubtless occurs mainly in the first and last hours of sunlight when the sun is low in the heavens and the concentration of atmospheric pollution tends to be high. In the period 1881-1885 the December-January sunshine in Central London was 20 per cent of that at Kew Observatory. Fig. 100 of Bilham (1938) indicates a general upward trend of the ratio of Central London to Kew winter sunshine until 1930-1935, while a revised and extended version of this diagram suggests that the ratio reached 55 to 60 per cent in 1940-1950. The ratio for June-July has increased from about 84 to more than 95 per cent. As there is no evidence of a systematic fall-off in sunshine at Kew since 1880 it is to be inferred that the atmosphere in Central London has become cleaner during the last 60 or 70 years, a conclusion consistent with the opinion of Bonacina (1950) and others that London fog in recent years has been less foul than formerly. Durst (1940) noted that during Sept. 1936-Mar. 1937 there were fewer thick fogs in Central London than in some suburbs and other neighbouring districts. This feature, probably mainly attributable to the thermal characteristics of a large built-up area, has also been remarked upon at other times, but is not without exceptions.

3. Distribution of fog in Great Britain

A broad picture of the incidence of fog in Great Britain is obtainable on reference to charts, on pp. 133-4 of the *Climatological Atlas of the British Isles* (1952), showing the average number of occasions of fog (visibility less than 1,100 yd) and thick fog (less than 200 yd) at 09 GMT during 1934-43 for the whole year and for October-March. Prominent features are the higher frequencies of fog and thick fog in the Clyde-Forth, Midlands of England and the London areas. For the year as a whole the average number of occasions of fog exceeds 30 over a large part of the Midlands from Warwickshire to North Yorkshire, and is rather more than 50 in the central part of this area; the corresponding values for the London and Clyde-Forth areas are about 30 and 20 respectively. A high proportion of the fog in the areas named occurs in the winter six months. The generalized charts cannot reveal the detailed influence of topography and other local circumstances on the liability to fog but suggest some broad association between the more frequent occurrence of fog and the more populous areas of the country. A similar tendency was noted in the investigation of winter fog and mist in 1936-7 (Durst 1940).

4. The fog of 5-9 December 1952

(a) *Preceding conditions*

Some features of conditions in the south-eastern part of England in the three months before December 1952 may be of interest. From September the monthly mean air temperature was below average by 1·5° to 4°F. The temperature in the ground was 1° to 3°F below average at a depth of one foot, and about 1°F below average at four feet. Precipitation in the area was well above average (50 to 100 per cent in places) in August and September, not greatly different from average in October, and again well above average in November (by 50 per cent or more in the London area and Essex). Successive snowfalls occurred in the last days of November in East Anglia and the Midlands. On the last day of November level snow was 10 in. deep at Whipsnade in Bedfordshire, with drifts up to eight feet deep in the vicinity. Snow remained on the ground at places in the Cotswolds area and probably in the Chilterns on the first few days of December.

(b) *General weather situation*

The synoptic situation and aspects of the fog in the London Basin have been discussed by Douglas and Stewart (1953).

Fog occurred at many places in England on 2 December but was swept away on the next day. By midnight 4-5 December almost all England was covered by the central region of an anticyclone which had moved south-east from near Iceland. At this hour, visibility was 1,100 yd or less in many places from Essex to the Severn area. The centre of the high pressure system moved slowly eastwards during 5 December and by evening of 6 December had linked with another high-pressure system on the Continent: Fig. 1 of Douglas and Stewart (1953). It was not until 9 December that, with the withdrawal eastwards of the ridge of high pressure, S to SW winds became established over all SE England.

Within an almost stationary central region of nearly uniform pressure and little cloud the stage was set for the development of fog. In the general London area pressure was remarkably uniform and the surface wind very light or calm throughout the four to five-day period. The continuance of these conditions for an unusually long period is regarded as the primary cause of a fog so persistent and so dense. Many parts of the country, other than the general London area, experienced fog during the period, e.g., Clyde, Tyne, Tees, a considerable area of Yorkshire, parts of Lancashire, other Midland counties, the Cotswold and Severn areas, East Anglia and locally in southern England. As far as is known, in no other area was the fog so continuously thick as in parts of the London region.

No information is available as to the magnitude of the surface inversion of temperature in the immediate vicinity of London. Fifty miles northward at Cardington, where there was little fog, temperature increased from the surface up to heights between 500 and 1,000 ft: the increase to 500 ft was about 10° or 12°F at times on 5 and 6 December and thus somewhat greater than the increase to 500 ft at Larkhill (Wiltshire) and Hemsby (Great Yarmouth).

Comment on the air temperature at four feet above the ground during the fog may possibly be relevant to some problems which arise from the incident. In the main, 6 and 7 December were the coldest days. On the 6th, temperature did not exceed 32°F generally in Greater London, and reached only 29°F at London Airport and Wisley (Surrey). At Hampstead (450 ft), however, the maximum temperature was 40° on the 6th and 37° on the 7th, and on the latter day 0·3 hour of sunshine was recorded. The lowest minimum temperatures in the period ranged between 24° and 27°F at most London stations, with readings of 20 to 22°F on the fringes, 19°F at Horley (6th), 18°F at Rothamsted (7th) and 15°F at Luton (5th).

(c) *Fog in the London area*

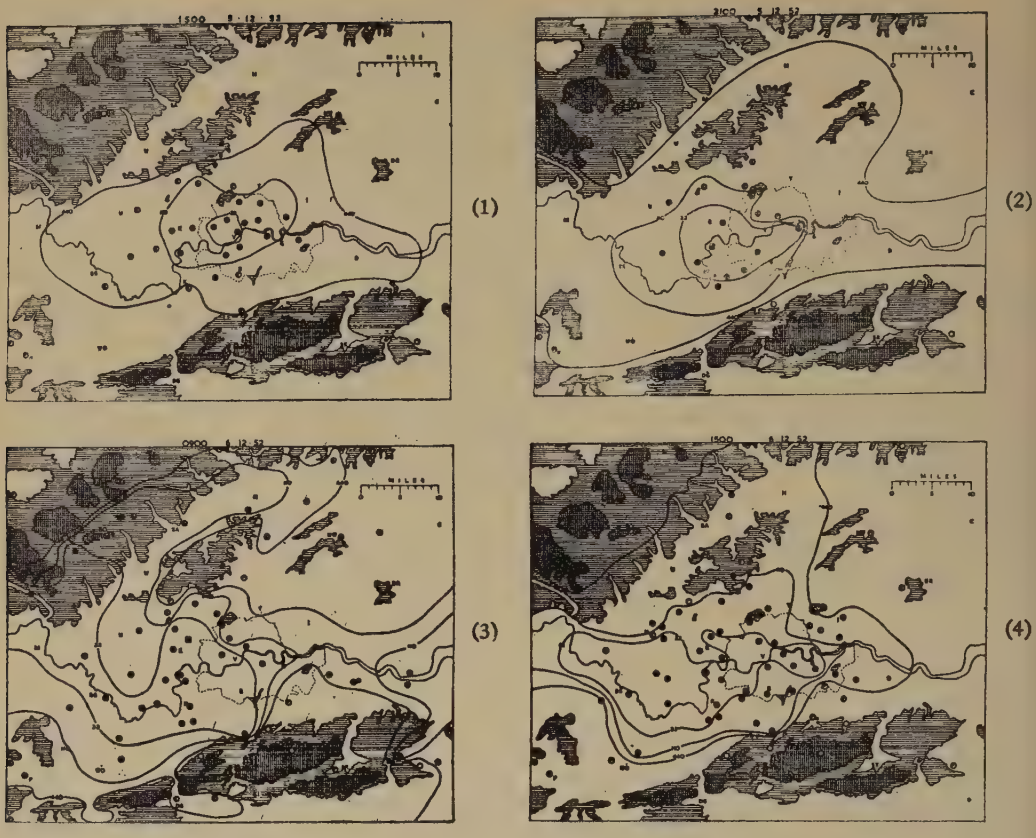
Bonacina (1953, a and b) and Meetham (1953) have called attention to and discussed certain features of the great fog.

In this section are presented some details on the extent and intensity of the fog in the London Area. The charts in Figs. 1-12, taken from an unpublished report prepared by Dr. K. H. Stewart, are based on observations made at the several synoptic reporting stations and at climatological stations (09 hr only) in the area, information supplied by the Automobile Association, the Royal Automobile Club, London Transport and the Port of London Authority, and also reports supplied some time after the event by Meteorological Office staff from their personal experiences at various times and places. Some of the information collected was not precisely comparable with that of the standard official observations. In view of this, together with the variability in the number of reports and the scarcity of information in some parts of the area, the isopleths drawn on the charts to show the distribution of fog of different intensities are rather tentative.

There were several reports of visibility less than 10 yd, notably on the evening of 7 December. At London Airport fog persisted from late on the 4th until early afternoon on the 9th with a break around 15 hr on the 8th; the fog was thick (visibility less than 220 yd) from the evening of the 5th until after noon on the 8th and again during the following night, with visibility less than 10 yd at times. At Kew Observatory the visibility was mainly below 22 yd from the evening of the 5th until 06 hr on the 8th. In Kingsway, Central London, fog persisted for about 114 hours and was thick or dense during 48 hours on 6th-8th.

The area within which visibility was below 22 yd appears to have increased in size and extended westwards to the lower Chilterns between the afternoon of the 5th and the morning of the 6th when it stretched also from near Harrow to Croydon. In the afternoon of the 6th the thickest fog occurred in a belt of varying width on either side of the Thames from the Staines-Maidenhead area to Greenwich. By the morning of the 7th visibility had improved (though to not more than 110 yd) westward of Ealing but was below 22 yd in an area extending from near Harrow to Ilford and from Croydon to Barnet. In the afternoon of the 7th the area of thickest fog was to the east of Barnet, Hampstead, Fulham, mainly to the north of the Thames, but dense fog recurred in southern and western districts of Greater London during the evening of that day. During the 8th the fog was more patchy; it was very thick in the afternoon and early evening in eastern London and down river, and also in the late evening in western London. By the morning of the 9th visibility was generally greater than 440 yd.

The general evidence, along with reports from Croydon Airport and the neighbouring high ground at Farthing Down and Sanderstead, suggests that the southern limit of the main fog area was roughly along the 250 ft contour of the North Downs. Biggin Hill (600 ft), Epsom Downs (500 ft) and Box Hill (Douglas and Stewart 1953) were above fog. To the north of London, the fog extended at times to the 400 ft contour of the Chiltern Hills. In the inner area, Shooters



Figures 1-4. Distribution of fog in the London area, 5-6 December 1952.

(5)

(6)

(7)

(8)

(9)

(10)

(11)

(12)

Figures 5-12. Distribution of fog in the London area, 6-9 December 1952.

Hill (400 ft) is reported to have been clear of thick fog at times, while Harrow Hill was mainly clear above 300 ft during the afternoon of 7th December. The height of the fog in Richmond Park on the 7th was estimated not to exceed 300 ft while the horizontal visibility was 100 yd in the Park (*Meteorological Magazine*, February 1953). Reports from aircraft landing at Farnborough and London Airport, during temporary clearance of the fog, gave the fog top at 250-300 ft with haze extending up to 2,000 ft. The tops of the stacks of Battersea Power Station (337 ft) were observed on one occasion to protrude above the fog. (Wilkins 1953).

5. Other fogs

Douglas and Stewart (1953) mention the London fog of 27 Nov.-1 Dec. 1948 as the occurrence of recent years which is most closely comparable with that of December 1952, and refer also to the dense persistent fog in the Glasgow area 16-21 Nov. 1925. It is well known that thick fog is by no means exceptional in Great Britain and it may be useful to record here for reference a list of some of the fogs which have been singled out for mention in the *Monthly Weather Report* during the period 1918-1952.

1918	31 Jan.	very dense locally in London (Brooks, 1918)
1921	27-28 Nov.	London traffic much dislocated
1923	24-27 Nov.	shipping and air traffic interrupted in SE England (Shaw and Owens 1925)
1923	2-8 Dec.	Birmingham and London affected 6 and 7
1924	6-15 Nov.	Glasgow area affected
1924	10-12 Dec.	62 hr of fog in London (Shaw and Owens 1925)
1925	10-12 Jan.	dense fog around London
1925	17-23 Nov.	many areas, especially Clyde-Forth
1927	11-17 Feb.	E and SE England, persistent in parts of London 11-13
1928	20-28 Feb.	London and SE England
1928	8-9 Nov.	thick in Clyde
1929	Jan	widespread 8-10, 20-23, 28-31
1930	Dec.	dense on Clyde on 3rd, more general 4-6, in southeastern districts on 15, traffic dislocated in London on 22
1931	Nov.	over wide area on 22 and 29-30, dense fog in Clyde on 16
1933	16-18 Jan.	continuous in Clyde
1934	Nov.	12-15 in Clyde, 19-25 in E and SE England, 19-21 persistent in London
1935	5-7 and 18-23 Dec.	atmospheric pollution at Kew, on 23, greatest for 11 years
1936	19-28 Nov.	notably persistent and thick in Rotherham, Manchester and Birmingham areas (*Meteorological Magazine*, 1936, pp. 252-6)
1937	Nov.	in many areas 1-8, 13-16, 18, 21-28
1937	25 Dec.	thick over large part of England, especially north London
1941	18-21 Dec.	continuous in Central London
1942	11 Nov.	dense in London
1944	Dec.	considerable and persistent in various areas 13 and 19-29; persistent in Central London 25-28 (Marshall 1952)
1947	6-7 Nov.	traffic dislocation in parts of England
1948	1-6 Mar.	unusually foggy for March
1948	22-30 Nov.	south Scotland and eastern and central England, continuous fog in Central London late 26 to 1 December
1951	13-14 Dec.	widespread and thick in London area, much of eastern and central England and the Clyde
1952	19-21 Jan.	dense at times in the Clyde
1952	5-9 Dec.	notable fog in London area

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551.510.42 : 551.575.5

Air pollution aspects of the London fog of December 1952

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Summary

This four-day fog caused at least 4,000 deaths and much illness; it therefore ranks as the most disastrous event of its kind known to have occurred anywhere in the world. For the first time in an incident of this kind extensive measurements were made of the daily concentrations of smoke and sulphur dioxide and of the general pattern of pollution throughout the area.

Mean concentrations of smoke and sulphur dioxide at 12 daily test-sites increased sharply on the first day of the incident and, after 3 to 4 days, averaged 1·6 mg/m³ and 0·7 parts per million respectively, or about 5 to 6 times normal. At individual sites in the central London area concentrations rose to 10 times normal.

There was a striking parallelism between the mean daily concentration of smoke and sulphur dioxide and the total number of deaths on those days but the latter returned the more slowly to normal.

A few data, relating to a severe London fog of 1948, indicated that the concentrations of pollution then reached about 50 to 70 per cent of the 1952 values.

The approximate distribution of sulphur dioxide is shown on a map which indicates zones of maximum pollution mainly in the riverside areas, and particularly Westminster and adjacent boroughs.

1. Introduction

No records are available to indicate the degree of pollution of London fogs of 40-50 years ago but there are many observers who aver that those fogs were no less persistent, dense or grimy than that which enveloped London between 5 and 9 Dec. 1952. Nevertheless the physiological effects of this most recent incident appear to have been unprecedented, for it has been shown (Logan 1953) that never during the last 80 years or so of London's history have so many as 4,000 deaths been caused by an occurrence of this kind. The disaster was without doubt the most serious of its kind in this or, so far as is known, in any other country.

The serious effects which the 1952 fog, or ' smog,' incident had on mortality and morbidity led to the suggestion in many quarters that some unusual form of pollution of exceptional toxicity was present in the air at that time. Whilst this possibility cannot be positively excluded, it is noted that (i) the effects occurred more or less simultaneously over an area of some hundreds of square miles and (ii) the lethal effects of London smog are by no means new; there have been similar incidents in the past when over 1,000 deaths were caused in London. In this latter connection it may be pointed out that some of the worst of these previous incidents occurred during the 19th century before the internal combustion engine added its contribution to London's pollution.

In considering the factors which caused the excessive accumulation of air pollution in the affected area it is interesting to compare London's 1952 incident with those which occurred near Liège in 1930 and at Donora, Pennsylvania, in 1948. In all three instances the fog or smog lasted about four days and, taking into account the size of the popu'ations involved, the mortality rates were of the same order of magnitude. There was however a significant topographical difference. The two earlier incidents occurred in narrow steep-sided valleys which thus presented a land barrier to the dissipation of the polluted air. In the Thames valley the fog reached from the North Downs to the Chilterns, a distance of between twenty and thirty miles, but, as will be shown later in this paper, the area of maximum pollution was confined to the central London part of the basin; towards the fringes of the fog-bound area there was relatively little pollution. Thus it appears that pools of heavily polluted and dangerous smog may persist in an expanse of relatively unpolluted fog without any confining land surface such as was provided by the sides of the valleys in the Liège and Donora incidents.

2. Measurements of pollution

For many years measurements of air pollution have been made in many parts of the country; since 1945 this work has been organized by the Fuel Research Station in co-operation with various local and other authorities. The number of measurements has increased considerably during

recent years; in consequence, information on the distribution of pollution is more detailed than it was even a few years ago, although it is still far from adequate.

The present investigation is concerned mainly with the pollution of the fog by smoke and sulphur dioxide; in addition to being important in themselves, variations in the concentrations of these substances are likely to give some indication of probable variations in the concentrations of other polluting substances not actually measured.

(a) *Daily measurements*

The mean daily concentrations of smoke and sulphur dioxide were measured at twelve sites in the Greater London area. The apparatus used for these determinations consists of an electrically-operated suction pump which aspirates the air to be tested first through a filter paper, then through a solution of hydrogen peroxide and finally through a meter. The amount of smoke caught by the filter paper is assessed from its blackness and the increased acidity of the hydrogen peroxide solution, as determined by titration, is calculated as sulphur dioxide.

It should be noted that the air enters the inlet tube of this combined smoke and sulphur dioxide instrument through an inverted funnel which prevents the entry of particles or droplets with a falling velocity of more than about 1 in./sec, i.e. larger than about 20 μ in diameter. The measurement therefore includes sulphur dioxide in the gaseous state and also that dissolved in the smaller fog droplets; it does not include the sulphur dioxide which may be associated with the larger water droplets.

Sites where daily measurements of smoke and sulphur dioxide concentration were made are listed in Table 1 in order of increasing distance from Charing Cross. Results are given for each 24- or 48-hr period between 2 and 10 Dec. 1952. Test periods were normally from noon to noon and the dates quoted are those on which the test periods ended. Average results for the month of December 1951 are also given as an indication of the normal concentrations at that time of the year when there was no serious fog condition.

It will be noted that the normal concentrations of smoke ranged from 0·44 to 0·12 mg/m^3 in the central and outer London areas respectively. The maximum smoke concentration reported was 4·46 mg/m^3 at County Hall, Lambeth, on 6 to 8 December; fairly high concentrations also occurred at the Westminster, Southwark, Greenwich and North Woolwich sites, with generally decreasing concentrations towards the more outlying areas. At individual sites the maximum concentrations were between three and ten times the normal values, the greatest relative increases being in the central area. The daily averages of all sites are plotted in Fig. 1, which shows a general increase during the first three days of the fog to about 1·6 mg/m^3, i.e., to about five times normal.

Normal concentrations of sulphur dioxide were between about 0·23 and 0·04 parts per million parts of air. The maximum concentration during the smog occurred at County Hall, Lambeth, on 6 to 8 December, when 1·34 p.p.m. was recorded; high concentrations were also recorded at Westminster, Southwark and the City, all within about two miles of Charing Cross. At individual sites the maximum concentrations were between three and twelve times normal. Average daily concentrations for the whole area, shown in Fig. 1, indicate a progressive rise during the first four days to a maximum of 0·7 p.p.m. or about six times normal.

A few data, which are available, for pollution during the severe fog which lasted from 26 November to 1 December 1948, show that on that occasion also the highest readings were obtained at County Hall, Lambeth, and in the City and Westminster areas. The mean concentrations of smoke in those areas were between 0·5 and 2·0 mg/m^3 and of sulphur dioxide about 0·5 p.p.m. At the other test sites in the City, Greenwich, Richmond, North Woolwich, Erith, Twickenham and Hornchurch, the concentrations of smoke were between 0·9 and 0·2 mg/m^3 and of sulphur dioxide between 0·4 and 0·1 p.p.m. In general the degree of pollution during the 1948 incident was between about 50 and 70 per cent of the corresponding values during the 1952 incident.

(b) *Approximate pattern of pollution*

It has been possible to obtain a more detailed picture of the approximate distribution of sulphur dioxide during the smog incident from the results of 117 instruments of the lead peroxide type which measure the rate of sulphation of a standard surface of lead peroxide over a period of one month. The high sulphur dioxide concentrations which prevailed during the smog incident of December 1952 increased the average concentration for the month at most sites by 50 per cent or more. These increases above normal have been used to calculate the average

relative concentrations of sulphur dioxide during the smog period at 117 sites in the London area, the values for December 1951 being taken as normal. The resulting pattern of pollution is shown in Fig. 2.

TABLE 1. DAILY SMOKE AND SULPHUR DIOXIDE CONCENTRATIONS, LONDON.

December 1951 and 2 to 10 December, 1952.

Test site (figures in brackets are distances in miles from Charing Cross)	Mean Dec. 1951	December 1952								
		2	3	4	5	6	7	8	9	10
					Fog days					
Smoke (milligrams per cubic metre)										
1. Lambeth, County Hall (0·5)	0·43	0·49	0·61	0·49	2·64	3·45	4·46	4·46	1·22	1·22
2. Westminster, Monck Street (0·8)	0·34	0·38	0·38	0·52	1·93	2·40	1·20	1·20	1·44	0·48
3. Southwark, Walworth Road (1·8)	0·44	0·78	0·85	0·49	1·44	3·95	2·66	2·66	3·95	0·68
4. City, Golden Lane (2·0)	0·20	0·21	0·26	0·26	1·00	1·02	1·06	1·06	1·10	0·22
5. Greenwich, Fuel Research Station (5·5)	0·34	0·18	0·36	0·36	0·99	1·09	2·06	2·06	0·95	0·19
6. Chiswick, Town Hall (6·0)	0·20	0·21	0·22	0·66	0·64	0·42	0·35	0·35	0·50	0·09
7. Leyton, Sidmouth Road (6·3)	0·49	0·35	0·57	0·43	1·12	1·55	1·66	1·66	2·05	0·46
8. Richmond, Kew Observatory (8·3)	0·24	0·28	0·26	0·23	0·69	0·63	0·51	0·49	0·34	0·14
9. Woolwich (North), Beckton (9·0)	0·43	0·27	0·79	0·41	1·47	1·62	3·28	3·28	0·92	0·26
10. Erith, Crossness (11·0)	0·15	0·11	0·34	0·14	0·44	0·64	1·00	1·00		0·25
11. Twickenham, N.P.L., Teddington (11·3)	0·15	0·30	0·31	0·24	0·63	1·44	0·67	0·67	0·48	0·06
12. Hornchurch, Billet Lane (15·5)	0·12	0·03	0·18	0·03	0·03	0·34	0·56	0·56	0·37	0·07
Sulphur dioxide (parts per million)										
1. Lambeth, County Hall (0·5)	0·22	0·16	0·22	0·14	0·75	0·86	1·34	1·34	0·47	0·47
2. Westminster, Monck Street (0·8)	0·16	0·15		0·24		0·99	0·63	0·63	1·23	0·99
3. Southwark, Walworth Road (1·8)	0·11	0·19	0·26	0·18	0·41	0·95	1·14	1·14	0·80	0·14
4. City, Golden Lane (2·0)	0·17	0·12	0·19	0·19	0·62	0·72	0·80	1·22	1·21	0·22
5. Greenwich, Fuel Research Station (5·5)	0·10	0·09	0·27	0·27	0·35	0·42	0·72	0·72	0·36	0·03
6. Chiswick, Town Hall (6·0)	0·09	0·05	0·02	0·07	0·34	0·85	0·31	0·31	0·17	0·02
7. Leyton, Sidmouth Road (6·3)	0·11	0·06	0·12	0·09	0·25	0·29	0·62	0·62	0·64	0·17
8. Richmond, Kew Observatory (8·3)										
9. Woolwich (North), Beckton (9·0)	0·23	0·02	0·20	0·08	0·33	0·23	0·67	0·67	0·21	0·11
10. Erith, Crossness (11·0)	0·04	0·01	0·04	0·01	0·04		0·08	0·08	0·16	0·03
11. Twickenham, N.P.L., Teddington (11·3)	0·07	0·66	0·21	0·23	0·32	0·89	0·57	0·57	0·14	0·02
12. Hornchurch, Billet Lane (15·5)	0·07	0·02	0·08	0·03	0·17	0·14	0·34	0·34	0·19	0·05

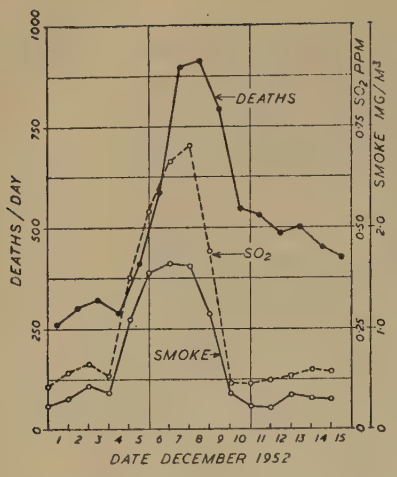

Figure 1. Daily air pollution and deaths.

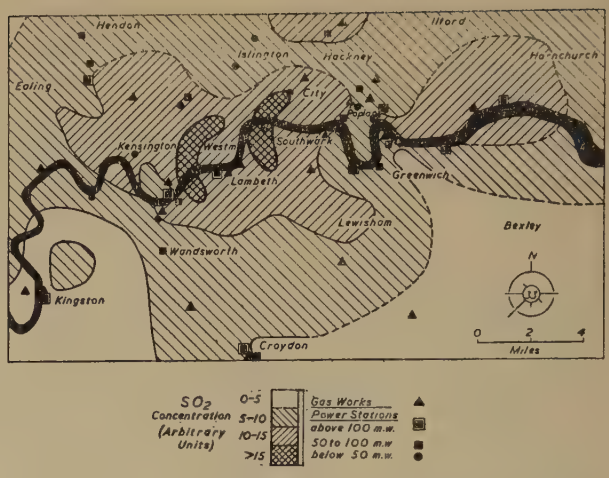

Figure 2. Approximate pattern of pollution by sulphur dioxide, 5 to 9 Dec. 1952.

It will be noted that two areas of maximum pollution occur in the areas represented by parts of Westminster, Battersea, Chelsea, Kensington, Lambeth, Southwark, Holborn and the City. A zone of lesser concentration lies along the valleys of the Thames and Lea and stretches for a few miles on each side of those rivers. In South London there were considerable areas of relatively little pollution.

Wind directions, as measured at Kew, are shown on the wind-rose in Fig. 2 which shows that there were 33 per cent of calms during the period and that for the remainder of the time the movements of air were mainly from the E, W and SW.

In connection with the above-mentioned evidence that during the smog period there was a centre of maximum pollution in Westminster and surrounding areas, the following two observations are appropriate : (i) the general shape of the pollution pattern as shown in Fig. 2 is roughly similar to that obtained during a non-foggy period in December, (ii) Absalom (1954) has shown that at 15 hr on the first day of the smog incident, the approximate centre of the affected area, where there was also the lowest visibility, lay in the neighbourhood of Westminster; this appears to confirm for that area the relationship between pollution and the tendency to dense fog formation.

3. Correlation with mortality

Fig. 1 shows, in addition to the pollution data previously mentioned, the total number of deaths from all causes which occurred each day in Greater London before, during and after the smog incident as quoted by Logan. Attention is drawn to the following features of this diagram : (i) the daily number of deaths at the period of greatest pollution reached about three times that recorded for the period immediately before the smog incident, (ii) the sharp upward trend in the death rate following the sharp upward trend of pollution at the commencement of the incident on 5 December and the sharp downward trend at its conclusion on the 9th; the chance of this remarkable parallelism being a coincidence is very slight, so it seems almost certain that we are dealing with cause and effect, (iii) the short time-lag between cause and initial effect amounting, possibly, to as little as 12 hr, (iv) the relatively slow rate of return of the deaths curve to normality during the aftermath of the smog.

4. Natural removal of pollution

Fig. 1 shows that the mean accumulation of sulphur dioxide in the air at the test-sites was at the rate of approximately 0·2 p.p.m. per day. Assuming this value applies to a volume of air covering 500 mi² of Greater London to a depth of 300 ft, which was the approximate depth of the fog layer, we have a total rate of accumulation of about 70 tons of sulphur dioxide per day.

The amount of sulphur dioxide released to the atmosphere in London during a cold gloomy smog when the consumption of fuel is probably a maximum, may be of the order of 2,000 tons a day (± 25 per cent).

The natural removal of sulphur dioxide from the smog therefore accounted for the equivalent of 1,930 tons out of each 2,000 tons emitted. On this basis, therefore, the death roll and other pollution effects of the fog are attributed to a decrease of only 4 per cent in the operating efficiency of the natural cleansing processes. The margin between safety and danger is indeed a narrow one !

References

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Physiological effects of smog

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Inclement weather affects most people who suffer from chronic bronchitis or asthma. The easiest way to understand its action is to consider first the normal anatomy of the lungs and then the changes which take place as a result of bronchitis. The bronchi divide many times within the lungs and eventually lead to the terminal units which are termed lobules, of which there are about 50,000 in normal adults. The air cells, or alveoli, which comprise the bulk of these lobules are lined by very thin vascular membranes across which oxygenation takes place. The total alveolar surface in health is equivalent to about half the size of a tennis court. Each lobule is supplied by a bronchiole which is the terminal branch of a larger bronchus, so that during each breath there are potentially 50,000 jets of air supplying a considerable surface of alveolar walls. The bronchi and bronchioles are covered with a thin layer of mucus and normally about 100 cm^3 of this mucus are moved up the bronchial tree by ciliary action in each twenty-four hours, the mucus eventually being deposited in the oesophagus and swallowed. During the process of inspiration, a considerable transference of moisture takes place from the bronchi, through the mucus, to the inspired air (see Table 1). In the English climate this amounts to about 400 cm^3 in 24 hr.

TABLE 1. Some factors in the humidification of inspired air

500 ft^3 air	Temp. (°F)	Rel. Hum. (%)	Moisture content (oz)
Under winter conditions	40	86	3
Under summer conditions	60	77	5
Reaching the alveoli	95	95	19

In bronchitis, and the changes are very similar in asthma, the bronchial and bronchiolar walls become inflamed and hypersensitive; also, there is an excessive production and secretion of mucus. The larger bronchi are for the most part of sufficient calibre to remain open, so that air during respiration can pass freely along them. The bronchioles, being only about one millimetre in diameter, are often obstructed. This obstruction causes damage to the lobules beyond, which may easily become the site of pneumonia, collapse or abscess formation. The progressive nature of chronic bronchitis suggests that each fresh attack damages further groups of lobules. In this way, the total number of effective lobules is steadily reduced so that in the final stages of the disease there may be only sufficient effective alveolar surface to maintain oxygenation at rest.

Both in bronchitis and asthma the bronchi and bronchioles are hypersensitive, and a large variety of irritants is capable of stimulating excessive secretion of mucus. Amongst climatic factors, fog is the most formidable stimulant and upsets the great majority of bronchitics, and

dampness affects almost as many; about half are affected by dry cold. The duration of irritation is important. Exposure to a fog for, say, half an hour may lead to distress for only a few hours. Should the exposure be prolonged to several days, not only is the immediate effect accentuated, but it is likely to be prolonged. Atmospheric pollutants enhance the effect of fog, although the exact ingredients of pollution which cause this have not yet been established. There is a very great difference in the amount of pollution in cities and country districts, and deaths from bronchitis vary in England with the amount of atmospheric pollution. It is tempting to think that if atmospheric pollution in cities were reduced to the proportion normally found in rural areas, the mortality from bronchitis in the country as a whole would fall by about 10,000 deaths a year – that is, one-third of the deaths attributable to bronchitis. This almost certainly is not true, because city dwellers are much more likely to contract respiratory infections by contact and many work in occupations which are injurious to the lungs, but which do not contribute materially to the total of atmospheric pollution.

The problem thus remains very complex. Bronchitic and asthmatic patients have sensitive lungs, but the sensitivity is in respect not only of adverse climatic factors, but also of atmospheric impurities and respiratory infections.

Discussion

Dr. R. Lessing : I wish to raise the question of the significance of the results of observations made under the aegis of the Atmospheric Pollution Research Committee of the D.S.I.R. The difficulty of correlating the weights obtained in the tests with the pollutants emitted into the air is due to an indefinite proportion of the pollutants being washed out by rain before the air reaches the testing point. Of the solid deposits – apart from the coarsest particles which settle spontaneously – only that portion brought down by rain reaches the deposit gauges. During dry spells the bulk of the fine particles tends to remain suspended in the air. On the other hand, the sulphur dioxide caught by the lead-peroxide instruments, and the smoke and sulphur dioxide extracted by the volumetric method represent only what is left in the air after a portion has already been washed out by rain.

An interesting and useful opportunity for estimating the respective proportions is provided by the observations during the excessively dry month of March 1953, to which Dr. J. Glasspoole referred in his survey of rainfall in *The Times* of 12 Jan. 1954.

For that month the lead-peroxide ' candle ' on the apex of the west roof of St. Paul's Cathedral gave a record figure of 8·82 mg SO_3 per 100 cm^2 per day, and that in the more sheltered position of the library roof 7·30. The latter value compares with 1·95 in March 1952 and an average of 2·95 for March in the 4 years 1949-1952. Assuming the 7·30 value for the dry month closely to approach the total sulphur-oxide content of the air and the sulphur-oxides pollution to have been constant during March of the preceding four years, the mean of the values found at that observation point in these more normal months of March would represent only 40·4 per cent of the sulphur oxides which had been originally present in the air. The assumption of constant emission is not strictly correct. In fact, a general rise in the figures over previous years was observed at St. Paul's throughout 1953.

A similar comparison of the results obtained by the volumetric method at County Hall gives on the same assumptions the following results :

	Rainfall (in.)	Smoke (mg/100 m^3 air)	Sulphur dioxide (mg/100 m^3 air)
March 1952	2·59	32·0	51·5
March 1953	0·51	53·0	72·4
Test yield 1952 compared with 1953		60·4%	71·1%

An appreciable portion of the pollutants thus escapes detection at the observing stations. The undetected portion of sulphur oxides and of particulate matter will, of course, have already been washed out by rain or, in dry weather, have been deposited on the ground at some distance from the observing station.

To visualize the magnitude of sulphur-oxides pollution in the country I have in a previous

discussion related the sulphuric acid derived from the sulphur of 200 million tons of coal consumed in a year to the total annual rainfall. If the acid emitted were to be mixed with the total water precipitated during the same period, the concentration of the resultant H_2SO_4 solution would be about 60 p.p.m., making due allowance for SO_2 blown out to sea and leaving out of consideration the unpolluted western parts of Scotland and Wales.

A calculation on similar lines applied to the densely-populated industrial areas, occupying 3,752 mi^2 and consuming two-thirds of the country's coal supply, gives a mean concentration of not less than 600 parts H_2SO_4 per million parts of water. This acidity of the aqueous precipitation, varying according to time and locality, affords a measure of the immense damage caused by sulphur emission.

Mr. E. Gold : I would like to make some observations on the concentration of carbon monoxide and sulphur dioxide at peak hours in streets with a very high density of motor traffic. In most places and under most meteorological conditions the concentration of carbon monoxide or sulphur dioxide due to the exhaust gases from motor vehicles is hundreds or thousands of times less than the accepted lower limits of the concentrations dangerous to mankind – except in or in close proximity to the exhaust stream. The gases are dispersed by turbulence and carried away by the wind. But in streets, between high buildings, in which there is exceptionally dense motor traffic, continuous for periods of an hour or more, the concentration of these poisonous gases may rise well above the danger limit in a meteorological calm i.e. when smoke rises vertically.

Such unfavourable conditions are not infrequent. They occur at times in Regent Street for example. I have myself noticed a feeling of lassitude after driving up it in the late afternoon on occasions when it was packed with vehicles with their engines alternately accelerating and idling. I always breathe a sigh of relief on passing Broadcasting House, as if I were emerging from a twentieth-century Black Hole. My experience led me to make some simple observations and rough calculations of the concentration arising from these engines. I was further led to estimate the heat produced and its effect on the temperature of the air and thereby on the distribution of the exhaust gases. I may say at once that if it were not for the scavenging effect of this heat really dangerous concentrations would almost certainly be reached in Regent Street and streets like it, when they are packed with motor traffic in the late afternoon or on a winter morning in a meteorological calm.

On occasions such as those mentioned there are, in a distance of half a mile, about 400 engines running, counting a bus engine as equivalent to two. I take the fuel consumption as about 2 gal/hr per engine, or about 14 lb. The exhaust gases will then be about 200 lb per engine per hour, or rather more than 2,500 ft^3 at N.T.P. For the 400 engines this gives about 10^6 ft^3/hr. (The engines move out of the street but others move in so that we may take the half-mile as continuously occupied by 400 engines). In a calm this exhaust gas will be mixed with the air in the street and above it. If it were uniformly mixed with the air in the large box formed by the street, the buildings and a lid at a height of 100 ft, the concentration would be about 5 per cent. If we allow a 2 per cent concentration of carbon monoxide in the exhaust gases (it may rise to 7 per cent or more) the concentration of monoxide in the mixture would be one part per thousand. A concentration of one per cent is stated to be practically immediately fatal and one part in a thousand fatal if continued for some time.

If half the fuel consumption was heavy oil by buses, the concentration of sulphur dioxide in the air in the box would be, by volume, about 2½ parts per 100,000 or 2½ times the maximum allowable continuous concentration specified in American regulations for men working on 8-hour shifts.

If we take the figures of the Beaver report, one lb. of petrol yields about three-quarters of a lb. of carbon monoxide in the exhaust gases. This gives a concentration of monoxide of 5 per cent in the exhaust gases and of 2½ parts per thousand in the air in the box.

These concentrations at the end of an hour would be additional to any which the air previously contained. I do not suggest that the exhaust gases would, in fact, be confined to such a box; but if there were no artificial heating of the air near ground level, there might well be a lid at 100 ft in winter or towards sunset in summer.

Fortunately the engines do provide artificial heating at the bottom of the box. They would, in the course of an hour provide enough heat, about 10^8 BTU, to raise the temperature of the air in the box by 300°F. Naturally radiation disposes of a substantial proportion of this heat but even on a most liberal allowance for this and the absorption of heat by buildings (1 BTU ft^{-2} min^{-1}, or 0·4 g cal cm^{-2} min^{-1}) this would not dispose of half the heat. There is therefore no possibility of a lid at 100 ft under the conditions specified.

If this heat, after allowance for the disposal of half of it by radiation, etc., were distributed so as to turn the air in the box and above it from a stable state, with a lapse rate one-third of the dry adiabatic rate, to a labile state with a dry adiabatic lapse rate, it would effect this up to a height of about 2,000 ft and the increase of temperature at street level would be about 8°F.

Regent Street is 80 ft wide between buildings. Owing to horizontal diffusion the box through which the heat is distributed will grow wider upwards and we shall not be far wrong if we take its effective mean width to be 110 ft (this assumes a spread of the one per cent limit equal to 30 ft at a height of 300 ft above the top of the buildings, a law of variation $z^{0.8}$ upwards and an exponential law of concentration horizontally). If the box is z ft high, the mean change of temperature in it is $2 \times 10^{-3}\, z$ and its volume is $2{\cdot}9 \times 10^{5}\, z$. The heat required to raise the temperature of 1 ft^3 of air by 1°F is about 1/52 BTU.

Hence, $5{,}800\, z^2 = 260 \times 10^7$ or $z = 2{,}100$ ft and the increase of temperature at street level is about 8°F.

I made some observations on a relatively calm afternoon, Monday, 18 Jan., but at times, 2.30 p.m. and 6.30 p.m., when traffic was only about one quarter of the maximum density of 4 cars every 9 yards. The temperature at 2.30 p.m. was 2°F higher in Regent Street than in Hanover Square, 47·0°F and 45·0°F respectively. Near halted traffic at traffic lights in Regent Street it was 47·6°F. At 6.30 p.m. when traffic was rather less the difference was 1°F, (45·6°F and 44·6°F) but 1½°F with Oxford Street (46·2°F). Thus a difference of 8°F at a time of maximum traffic is not unlikely.

If the exhaust gases were distributed through 2,000 ft of atmosphere, the concentration diminishing exponentially upwards to a value, at 2,000 ft, one hundredth of the surface value, the concentration at the surface would be about one part per 100. The concentration of carbon monoxide would be about one part in 5,000 on the 2 per cent basis or about one part in 2,000 on the Beaver report basis. The sulphur dioxide would be one part in 200,000 or about 4 times the concentration measured in December 1952.

As carbon monoxide is slightly lighter than air while sulphur dioxide is about 2¼ times as heavy as air, the addition to the street-level concentration of these gases due to the discharge of flue gases from chimneys would tend to be less for the carbon monoxide than for the sulphur dioxide.

If we make a rough guess of the solid fuel, burned in fires, stoves or boilers in the buildings of the street, as 400 cwt in 10 hr, the amount of carbon monoxide emitted in one hour would be about 7 cwt taking the figures of the Beaver report as basis. This compares with about 40 cwt from the motor vehicles.

I recognize that the carbon monoxide from Diesel engines is less than that from petrol engines. Also I recognize that the sulphur dioxide may be partially turned into sulphuric acid.

My main object in making these calculations is to see if the order of magnitude of the concentrations disclosed is high enough to warrant the trouble involved in taking and analysing samples of air at street level in the meteorological and traffic conditions indicated. The result confirms, I think, that ' spot ' samples should be taken. I emphasize that they should be in the right places at the right times and not averages over a period covering other traffic and meteorological conditions.

DR. H. JOULES : Although there are many aggravating factors in chronic bronchitis, atmospheric pollution is probably the most important. The ' smog ' of December 1952 caused a minimum of 5,000 deaths and many more were indirectly attributable to it. As doctors we should ask if there is a possibility of similar conditions recurring and if preventive action could be taken. There is no doubt that a comparable fog could recur and we should emphasize that the worst areas are in the centre of London where there has been a fall in population of one million since 1931.

Meanwhile the consumption of coal at power stations has increased by at least 400 per cent. Sulphur dioxide is one of the most lethal products of these stations; it could be washed from the smoke at a comparatively small cost.

I hope that correct emphasis will be placed on these factors by the Beaver Committee in its final report. The policy of burning more and more coal in the centre of London is absurd and we should cease to make an open sewer of London's air. We breathe from that sewer and it should contain air as free from taint as the water in the mains.

The major blame seems to attach to large coal-burning authorities and not to the diminishing number of domestic grates.

MR. A. J. WHITEN : The fog of December 1952 has been repeatedly described as the greatest disaster of its kind on record. I don't want to pit my memory against official records,

but I wonder how complete these records were at the beginning of the century. I have memories of London for over 50 years – there are those present with longer memories – and I distinctly remember black fogs, in which you could walk for half a mile and return with a black face; fogs in which you would stand at the base of a lamp-post (they were then about half the height of most lamp-posts of to-day) and not know whether or not the lamp was alight. These fogs became a thing of the past, and we had a spell of yellow ' pea-soupers,' which have become rarities. It is recognized that thousands died as a result of the smog of December 1952, but what records have we to show how many perished similarly 50 years ago ? More old folk died in winter than in summer, and it was left at that. There may have been worse disasters which were unrecognized as such. It is my business to feel my way through these London fogs; I am one of the bronchitics mentioned by the medical speakers and have been for some 35 years. For the benefit of the medicos, I may mention that a white fog gives my lungs no trouble, but as soon as it turns yellow I have to retire.

Another point of interest to me is the stress thrown on the comparative scarcity of fogs in the neighbourhood of the Isle of Dogs, and the relative lightness of the fog, on the occasion under review, in this district. So far as my memory goes, this used to be one of the most notorious fog-spots in the country, years ago. During the Blitz, this district, once resided in by many thousands of working-class folk, was almost completely destroyed – probably the most complete clearance in London. The result has been the vanishing of a huge volume of smoke from domestic fires, often blamed for fog in industrial areas. Perhaps a comparison between the fog-maps of the present and, say, the early 1930's, might throw some light on this problem.

Mr. L. C. W. Bonacina : I think we should endeavour to preserve a sense of proportion in relation to the smog of December 1952, especially in view of the regrettable absence of atmospheric pollution records for the nineteenth century. Certainly, as regards smoke, no one who knew the really terrible black fogs of Victorian times would allow that there is the same weight of carbon in the air today, nor would he expect there to be. On the other hand, SO_2 from the modern power house and CO from the internal combustion engine are new factors in the situation today, and if Mr. Gold's representation is applicable they should go far towards explaining a perplexing anomaly.

I should like to know whether smoke concentration tends to run parallel with water-fog density. The really dense patches of water-fog with visibility down to a few yards are always localized, and if these patches are also the most polluted they would support the idea of dangerous pockets about which I wrote in *Weather* (Nov. 1953).

Lastly I would like to emphasize that London with its far longer record of smogs than other great cities at home and abroad is at least fortunate in lying in an open, broad, well-ventilated valley where fogs are highly susceptible to meteorological changes and dangerous inversions easily dissipated.

Dr. A. R. Meetham : Dr. Wilkins asked why there were only 250 tons of SO_2 over London in the great fog, compared with 8,000 tons probably emitted. I think the answer is simply that SO_2 is removed fairly quickly from the atmosphere by a form of deposition. The ventilation of a room, for example, is controlled by the number of minutes per air change; we are concerned instead of air changes with the number of minutes before deposition.

In a note in the *Q.J.* for January 1954 I estimated that the time before deposition of SO_2 in the London fog was 6 hr and, for smoke particles, 13 hr. The longer time for smoke explains why its concentration was the same as for SO_2 although it was being emitted at only half the rate.

It is important to know why pollution is deposited in fog at such a high rate, viz., about twice as rapidly as in average weather. No observations are as yet available that will help, but like Mr. Gold I have been doing some calculations and getting some peculiar results. Obviously the deposition of pollution must depend on the surface area available with which floating particles or molecules can collide. The fog droplets themselves present a surprisingly large surface area. Considering the number and size of droplets over 1 m^2 of ground, I find they have a total surface area of 46 m^2. If both SO_2 and smoke collect on droplets this would explain why they do not stay in the free air for a much longer time. Similarly the surface area of smoke over 1 m^2 of ground is about 2·5 m^2 and this would attract SO_2. I do not believe the ordinary measuring apparatus measures smoke, or SO_2, attached to fog droplets; or that it measures SO_2 attached to smoke. May I urge that apparatus be set up to measure *all* the pollution in a sample of air and fog during foggy weather ?

The London disaster has brought it home to many of us that observations in average conditions are in several ways less important than intensive observations in extreme conditions.

Mr. C. J. Regan : Since the question of the effects of motor traffic has been raised by Mr. Gold I should like to say that there is already some information on this subject. It was investigated about 25 years ago by the L.C.C. in connection with the ventilation of the vehicular tunnels under the River Thames at Blackwall and Rotherhithe. I published a paper in 1932 entitled ' Motor fumes and atmospheric pollution ' (*J. Soc. Chem. Ind.*, **51**, p. 605) which summarized the considerations involved. The carbon-monoxide limit eventually adopted was 250 parts per million.

It should be pointed out that, whereas the exhaust gas of a petrol-driven vehicle may contain CO up to about 5 per cent, that of a Diesel which is properly adjusted should contain only an insignificant proportion of this gas, although there will be present other objectionable compounds such as oxides of nitrogen, aldehydes, etc. This subject of the composition of exhaust gases deserves much further study as does also the biochemical nature of the resulting reactions proceeding in human metabolism. In relation to the subject of air pollution and the effects of the gaseous constituents, the question of possible effects on the respiratory enzyme system seems to me to be potentially of prime importance.

Mr. G. Nonhebel : The charts of fog density shown by Mr. Absalom indicate that the densest fog did not spread westwards much beyond Central London and that there was less dense fog in the East End. I believe that there was a westerly wind of about 1 to 2 mi/hr during the fog. One would have expected therefore that the smoke and fog would have built up to its maximum density in the East End, particularly as there is there a greater density of houses and factories. If these facts are correct, have the meteorologists any explanation of the ventilating currents that must have come in to thin the fog over the East End ?

Mr. G. Spurr : It has been suggested by a previous speaker that power stations may be responsible for the increased intensity of smog. I should like to quote a few figures in their defence. Of the 200×10^6 tons of coal burned annually in this country, only 36×10^6 tons are used in the generation of electricity. This means that power stations, at worst, could be held responsible for only 18 per cent of the total smoke pollution from the produced combustion of coal. As it is, this percentage must be much less, because of the way in which coal is burned at power stations equipped with dust collectors having an efficiency of more than 95 per cent. Moreover, this small percentage is emitted from chimneys 200 to 300 ft high, whereas smoke from domestic chimneys and locomotives is emitted at near street level.

It is clear therefore that the contribution by power stations to the general pollution of the atmosphere must be a small fraction of the whole, so much so, that if the power stations were closed down the difference in smog intensity would not be significant.

Mr. R. G. Veryard (*communicated*) : It would appear from the preceding discussion, and from what has already been written on the subject, that the abnormally high number of deaths from respiratory disease in the foggy spell of December 1952 must have been due to a combination and cumulative effect of several factors rather than to one factor alone, and that not all the factors were meteorological or environmental – not that I am suggesting for one moment that the general pollution of the air need not itself be regarded with very great concern.

A possible factor which has been mentioned by several research workers is that of coldness. Writing on the influence of cold on the health of the inhabitants of London, Heberden (1797) drew attention to the startlingly high number of deaths in the severe January of 1797, when temperatures were well below freezing, in comparison with the number in the preceding January which was some 20° warmer. The following quotation from Heberden may well hold good today :

> If there be any whose lungs are tender, any whose constitution has been impaired either by age, or by intemperance, or by disease he will be very liable to have all his complaints increased and all his infirmities aggravated by such a season.

Many years later Young (1924/5) made a study of the influence of weather conditions on the mortality from bronchitis and pneumonia in children and he found that the mortality was most intimately related with the temperature of the preceding week – the lower the temperature the greater the fatality from bronchitis.

Russell (1924, 1926) also investigated the influence of fog and temperature on mortality from respiratory disease and he came to the conclusion that, although fog had a decided influence in some weeks, yet on the average, a low temperature exercised the more dominant influence.

Wright and Wright (1945/6) also made a study of the influence of seasonal conditions upon the mortality of infants from broncho-pneumonia in the metropolitan boroughs and obtained results in close agreement with those of Young. Indeed they arrived at quite high values of correlation co-efficients between broncho-pneumonia and temperature both for infants under two years

and for persons over 55 years, but they pointed out that the relationships were not strictly linear – the mortalities for both groups rising rapidly when the temperature fell below 40°F.

During the foggy spell in London in December 1952, and in the Donora and Meuse incidents, the temperature was well below 40°F. In fact, in December 1873 when there was an extensive fog lasting several days, and associated with which there was a general death-rate in London only slightly less than for the incident of December 1952, the mean temperature was below freezing for three consecutive days.

It is known of course that fog and low temperature often accompany each other but it would be interesting to find out whether there has ever been any marked increase in mortality from respiratory disease when there have been very low temperatures but no fog or vice-versa. May not humidity itself be a factor ? It is believed that in Canada, which has a very cold but relatively dry winter, and also in Scandinavia, the incidence of morbidity and mortality in respect of respiratory disease is low.

It is understood that, as far as the corrosion of metals is concerned, the effect of sulphur dioxide in the presence of particulate matter is greater with increased humidity, May there not be a similar process in respect of human beings ? When Aitken (1923) carried out his laboratory experiments to ascertain why, under certain circumstances, fog should thicken at sunrise, he came to the conclusion that in the presence of other impurities sulphur dioxide could, under the influence of sunlight, become an active nucleus producer. He suggested that both hydrogen peroxide (produced by the ultra-violet rays of the sun) and possibly ozone might be the oxidising agents converting sulphur dioxide to sulphur trioxide (this in turn leading to the production of sulphuric acid) but he also said that ' it is probably the carbon monoxide of imperfect combustion that is the partner in the formation of nuclei.' In drawing attention to the presence of carbon monoxide, Mr. Gold may have given us a clue to one of the reasons for the density and persistence of the London fog in December 1952 because, as suggested by Douglas and Stewart (1953), the abundant nuclei may have produced innumerable small and relatively stable droplets. Indeed one wonders how much sulphuric acid was present in the droplets !

' Smog ' is becoming an increasing menace but it is not new. As Russell reminds us, Evelyn, one of the founders of the Royal Society, described it as the ' hellish and dismal cloud of sea-coal which maketh the City of London resemble the suburbs of Hell.' Let us hope something will be done about it soon.

References

Aitken, J.	1923	Collected Scientific Papers, Cambridge.
Douglas, C. K. M. and Stewart, K. H.	1953	*Met. Mag.*, **82**, p. 969.
Heberden, W., Jnr.	1797	*Phil. Soc.*, London, XVIII.
Russell, W. T.	1924	*Lancet*, **207**, p. 335.
	1926	*Ibid.*, **211**, p. 1,128.
Wright, G. P. and Wright, H. P.	1945/6	*J. Hyg.*, Cambridge, **44**, p. 21.
Young, M.	1924/5	*Ibid.*, **23**, p. 151.

Mr. H. W. L. Absalom (*in reply*) : Two speakers have remarked on the apparent relative absence of dense fog in the eastern parts of London. This may be largely due to the variability, as to place and time, of the number of reports available. There were few reports of any kind from eastern London for the evenings of 5, 6, 7 December but, as the charts show, there is no doubt that dense fog occurred in that area in the afternoon and evening of 8 December. Also, Mr. Bonacina (1953a) was told that in the Isle of Dogs, ' you couldn't see your feet.' During most of the fog period the surface air motion in London was extremely slight and very variable in direction : a light drift from between south and west became rather more general during the afternoon of 8 December but died away at night.

It is fairly commonly held that modern fogs in London are less black and noxious than those of fifty or more years ago. It is doubtful whether the nature of the observational records of fifty years ago would permit a direct and satisfactory comparison to be made between the intensity of fogs in those days and the intensity of fog in the last thirty years, though the matter may be worth closer examination. Evidence from sunshine records (see p. 262) suggests that the obscuring power of the atmosphere of Central London has decreased in the past seventy years.

Dr. E. T. Wilkins (*in reply*) : I wish to say in reply to Dr. Lessing that the deposit gauge measures the substances which settled, including those brought down by the rain; sulphur dioxide and smoke which did not settle are measured by the lead-peroxide ' candle ' and the volumetric smoke and sulphur-dioxide apparatus. The differences between certain results which

have been quoted by Dr. Lessing could be caused by factors other than rainfall. One of these factors is the direction of the wind relative to the main sources of pollution, so that, on the basis of observations at a single site, it is not possible to assess changes in pollution over an area. To do this it is necessary to consider the general pattern of pollution as determined from a considerable number of suitably located instruments.

Referring to Mr. Gold's calculations I note that, on the basis of certain assumptions, the air in a street congested with motor traffic might reach concentrations of carbon monoxide of between 200 and 500 parts per million. These values could be compared with that of 34 p.p.m. determined by Mr. Regan for London streets in 1932 and quoted in the paper referred to in his contribution to the present discussion. If increases in the carbon monoxide content during fog were comparable with other increases, values of the order of those given by Mr. Gold are possible.

I agreed with much of what Dr. Joules said. If the medical authorities were satisfied that sulphur dioxide in the atmosphere was responsible for the loss of life and health, then the cost of flue-gas washing, or other means of maintaining a purer air, would not be begrudged.

In reply to Mr. Whiten, many observers agree that the fog of December 1952 was not so black as many of those of the past, but it is certain from the official records that the number of deaths caused by the more recent smog exceeded those of any similar incident during the last 80 years or so. With regard to the slightly smaller amount of pollution shown on the map, for the Isle of Dogs area, I am interested in the suggestion that this might be caused by the relatively sparse population in that area since the war.

Dr. Meetham has given some interesting calculations on the average life of pollution during fog and normally. It is true that the ordinary measuring apparatus does not measure the sulphur dioxide associated either with particulate matter or with coarse water droplets; it does, however, measure all the gaseous sulphur dioxide together with most of that gas which is dissolved in the finer water droplets. These smaller droplets (less than about 20 μ) pass into the apparatus where they evaporate, owing to the higher indoor temperature, liberating the sulphur dioxide in gaseous form.

Dr. N. Oswald : In reply to Mr. Veryard, I entirely agree that cold and damp are factors, but it is a combination of cold and damp and atmospheric pollution which is the thing most likely to upset bronchitics.

Proceedings at meetings of the Society

Ordinary Meeting, 20 January 1954

The following candidates were declared to have been elected that day by the Council :

Fellows

Blake, Brian Leigh, B.A., Peasley Cross Post Office, Peasley Cross Lane, St. Helens, Lancs.
Butler, Francis Herbert Culverhouse, M.A., M.Sc., Ravensmead, Keston, Kent.
Evans, Capt. Frederick John, Northolt Airport Club, Ruislip, Middlesex.
Marshall, Prof. John Stewart, Ph.D., Department of Physics, McGill University, Montreal 2, P.Q., Canada.
Roach, William Theodore, M.Sc., D.I.C., Kentish Lane, Nr. Hatfield, Herts. (*transfer from Student Membership*).
Stark, Robert Gordon, M.A., Meteorological Service of Canada, Gander, Newfoundland (*transfer from Associateship*).
Watson, William, Braehead, Cairneyhill, By Dunfermline, Fife.

Foreign Members

Fleagle, Assoc. Prof. Robert G., A.B., M.S., Ph.D., University of Washington, Seattle 5, Washington, U.S.A.
Huss, Abraham, M.Sc., Department of Meteorology, Hebrew University, Jerusalem, Israel.
McPherson, Capt. Daniel E., B.Sc., M.A., Box 495, Patrick Air Force Base, Florida, U.S.A.

Associates

Chaplin, Gordon H., 100 Dunvegan Road, Toronto, Ontario, Canada.
Evenson, Allen Arnold, Box 22, Gander, Newfoundland.
Huschilt, John, 33 Bayfield Avenue, Hamilton, Ontario, Canada.
Kashtan, Solomon, 5409 Park Avenue, Montreal, Quebec, Canada.
Mereu, Robert Frank, District Aviation Forecast Office, Malton, Ontario, Canada.
Lategan, Jacobus Francois, Box 201, Heidelberg, Transvaal, South Africa.
Nicholson, Norman Leon, M.A., Ph.D., Manotick, Ontario, Canada.

Student Members

Ball, Peter, Cottesmore, Kings Way, Harrow, Middlesex.
Forsdyke, Donald, 237 High Street South, Dunstable, Beds.
Nelis, Richard, 44 Grangeville Gardens, Finaghy, Belfast.

The meeting took the form of a discussion on ' Smog ' with particular reference to the 1952 disaster. Some 150 Members and visitors were present and the opening speakers were :
Mr. H. W. L. Absalom (Meteorological Office) : ' Meteorological aspects of smog.'
Dr. E. T. Wilkins (Fuel Research Station) : ' Air pollution aspects of the London fog of December 1952.'
Dr. N. C. Oswald (St. Bartholomew's and Brompton Hospitals) : ' Physiological effects of smog.'
The discussion is reported on p. 261 of this issue.

Repeat of Popular Lecture, 4 February 1954

Mr. C. D. Ovey, B.Sc., being in the chair, Mr. J. S. Sawyer, M.A., repeated his popular lecture on ' Weather forecasting,' in the Conference Hall of the L.C.C., at County Hall. The lecture was again open to sixth-form students, of whom over 400 were present by arrangement with the education authorities of the London, Buckinghamshire, Essex, Hertfordshire, Kent, Middlesex and Surrey County Councils.

Ordinary Meeting, 17 *February* 1954

The following candidates were declared to have been elected that day by the Council :

Fellows

Harper, Robert David Mauleverer, B.Sc., The Cedars, Bulmer, Sudbury, Suffolk.
Hustwick, Kenneth, F.R.I.C., A.M.Inst.F., Bemerton, Woodlands Road, Camberley, Surrey.
Jenkins, Idwal, 1 Lodge Green Road, Corby, Northants. (*transfer from Student Membership*).
Lake, David Brian, B.Sc., D.I.C., 15 Brantwood Road, Herne Hill, London, S.E.24. (*transfer from Student Membership*).
Langleben, M. Phillip, Ph.D., McGill University, Montreal 2, P.Q., Canada.
Musk, David Rolin, B.Sc., 1 Market Street, Aylesbury, Bucks. (*transfer from Student Membership*).
Turner, Wilfred Charles, M.D., D.P.H., 29 Blenheim Road, London, S.W.20.
Thorburn, Plt. Off. John Elworthy, R.A.F.V.R., c/o 11 Chester Street, Middlesbrough, Yorks.
Welburn, Samuel, Netherpenns House, Priestgate, Nafferton, Driffield, E. Yorks.

Foreign Members

Cunningham, Robert M., Sc.D., Mail Box 108C, R.F.D. 1., Concord, Mass., U.S.A.
Inoue, Eiichi, Sc.D., Department of Oceanography, Texas A and M College, College Station, Texas, U.S.A.
Peterson, A. Delbert, S.B., c/o Pan American World Airways, 150 Marunouchi Building, Tokyo, Japan.

Student Members

Curtis, Rodney Allan Parry, 89 Chadacre Road, Stoneleigh, Ewell, Surrey.
Leaf, Geoffrey Glanville, 305 Beverley Road, Anlaby, E. Yorks.

The following papers were read and discussed :

(1) ' A comparison of hemispheric circulations with particular reference to the western Pacific ' by W. J. Gibbs (*Q.J.*, **79**, p. 121). The paper was presented by Mr. J. M. Craddock.

(2) ' The dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere ' by J. Smagorinsky (*Q.J.*, **79**, p. 342).

(3) ' Some downstream effects associated with large-amplitude troughs in upper flow patterns ' by C. V. Smith and A. G. Forsdyke (*Q.J.*, **79**, p. 414).

High altitude rocket research. By Homer E. Newell, Jr. New York (Academic Press), 1953. Pp. XIV, 298; 93 Figs. (including photographs), 37 tables. $7.50.

This is a book that may be read with interest and pleasure both by the specialist and by the layman who avoids the theoretical portions. The author, who is Head of the Rocket-Sonde Research Branch of the Naval Research Laboratory at Washington, D.C., describes in great detail and with the utmost clarity the use of rockets for upper atmospheric research, beginning with captured V2's and continuing with American-designed Aerobees and Vikings. The reader cannot but be impressed by the skill, ingenuity and patience with which the author and his colleagues have tackled the immense difficulties attending the recording of upper atmospheric characteristics by delicate instruments carried at great speeds in a vehicle which vibrates, spins and yaws and whose surface temperature may be raised by friction through more than 300°C.

Since German wind-tunnel measurements had shown that ambient pressures exist during flight just ahead of the tail fins, gauges were mounted in this position. Ambient pressures may also be obtained from measurements made by gauges placed in other positions by using the Taylor-Maccoll formula. Probable errors of less than 10 per cent are claimed for heights below 70 km, but at higher levels accuracy is progressively reduced by the yawing of the rocket, by the escape of occluded gases and presumably, though it is not discussed, by instrumental difficulties especially at the lowest pressures.

Once the pressure-height variation is obtained, temperatures may be computed using the hydrostatic equation, provided the average molecular weight of the air is known. Densities may similarly be calculated but, though here molecular weight is not involved, the necessary differentiation magnifies errors present in the pressure-altitude measurements and other methods are preferable. The method favoured is to measure stagnation pressure at the nose of the rocket and to derive from it the ambient density of the undisturbed air by using the Rayleigh supersonic pitot-tube formula, which is applicable for altitudes where the mean free path of the air molecules is small. Density at 219 km was found to be within 20 per cent of 10^{-7} g m^{-3}, which is less than one-tenth of the hitherto assumed value. The Rayleigh equation may also be used to determine temperature by measuring pressures at two points on the nose of a supersonic rocket. The theory of each of these methods is given and other possible ways of measuring pressure, temperature and density are briefly discussed. Tables of smoothed values of these three elements, prepared by the Upper Atmospheric Research Panel are reproduced here along with a discussion of the method of their preparation. Rocket methods of measuring winds in the upper atmosphere are briefly discussed, though apparently few satisfactory measurements have been made.

Perhaps the most rewarding of rocket measurements are those of solar radiation, made using specially designed spectrographs, various photosensitive materials and photon counters. Though considerable portions of the spectrum remain unexplored, the success already achieved is surely a harbinger of exciting advances yet to come. Ozone concentrations have been measured up to a height of 70 km and have been found to agree with theoretical curves for heights above 35 km, each showing an approximately exponential decrease with altitude. Only on one occasion has evidence of the complete dissociation of oxygen above a height of about 100 km been obtained; there is as yet none concerning the dissociation of nitrogen. Despite the great difficulties involved in the collection of air samples and in their analysis, it seems clear that the analysis made by Professor Paneth and his colleagues at Durham indicates that the atmosphere is well mixed below 60 km.

The theory underlying the direct measurement of ion and electron densities in the ionosphere is given and the experimental methods are described, as well as the process of detection of current sheets by the observation of discontinuities in the variation of the geomagnetic field with height. The field was measured by installing in the nose of the rocket a total field magnetometer which provided an output proportional to the square root of the sum of the squares of the intensities in three mutually perpendicular directions, thus rendering the measurement independent of the orientation of the rocket.

There follows a chapter on cosmic rays and their investigation by rockets. During the free fall of the rocket, the gravitational field necessary to clear the Wilson chamber of droplets after expansion is absent so it is necessary to arrange for centrifugal clearance by rotation of the chamber, or of the rocket itself, between expansions.

Finally, there is a short account of high-altitude photography, the spectacular results of which are already well known.

J.P.

Physics of the stratosphere. By R. M. Goody. Cambridge Monographs of Physics, Cambridge University Press, 1954. Pp. X, 187; 61 Figs., 17 tables. 25s.

It is stated in the Preface that this book is written primarily for the physicist ' who is interested to learn something of the progress of a subject outside his own field of study,' rather than for the geophysicist or meteorologist. There is, however, no doubt that meteorologist and geophysicist will read this account of recent work on the stratosphere with both interest and profit. We may say at once that though we shall have to refer to some serious sins of omission we have not found any serious sins of commission. The term ' stratosphere ' is used, in a sense which is unusual for British workers, to include the whole atmosphere from the tropopause (at some 10 to 15 km) up to the centre of the supposed cold region nearly 80 km above the ground,

Some 200 references to original papers are given at the end of the book, which should indicate that the book is really representative of present knowledge. Further, the fact that the majority of these papers have been published within the last ten years is an indication of the activity in this branch of geophysics.

The short introductory chapter gives an account of the discovery of the Stratosphere by Teisserenc de Bort at the end of the last century and a brief account of ' Tools ' i.e., aircraft, balloons and rockets, and is probably the least interesting chapter in the book.

The second chapter, on Temperature, compresses into 36 pages an account of the temperature distribution, both vertical and horizontal, of this deep region of the atmosphere. In order to do this the whole of the work of W. H. Dines is omitted, his name not even appearing in the Index. While knowledge of the temperature of the lower stratosphere is now fairly well established and is not so ' exciting ' as that, which is now actively being gleaned, of the higher levels, we feel that the ' physicist ' for whom the book is written would have got a far wider grasp of the conditions in the upper air if the account had been much fuller. This is by far the most serious sin of omission and the author seems to have some uneasiness on this score since he states ' A vast body of data exists upon this region, but it would not be appropriate here to give it the detailed attention it merits.' It is difficult to understand why it is not ' appropriate.'

After the brief account of the temperature throughout the region which can be reached by balloons, an account is given of the way in which the temperature can be inferred from observations of ' abnormal ' propagation of sound waves, from a study of meteors and from pressure measurements made on rockets (the high speed making it impossible to measure the temperature directly). Finally short accounts are given of ' confirmatory evidence ' from noctilucent clouds at some 80 km, from solar spectroscopy, from atmospheric oscillations, from auroral spectra and from radio measurements. The description of these is both clear and adequate.

The third chapter, on Composition, contains a full account of the measurements of water vapour, owing to its great importance in atmospheric radiational equilibrium. This short chapter is concluded by an account of the determination of the ratios of light and heavy gases at different heights which is important as indicating to what height mixing extends. In view of the later analyses by Paneth and his co-workers of samples of atmospheric air collected at great heights by the U.S. workers on rocket flights, it seems unnecessary to have included the earlier work with balloons. The rocket samples indicated no change in composition up to some 65 km and thereafter a rather rapid increase in the proportion of the lighter gases. This result is taken as reliable though there now seems to be much doubt whether some artificial separation of gases does not occur when the vessel is opened under such low pressures and when passing so rapidly through the air.

Atmospheric ozone is not included in the chapter on composition but has a whole long chapter to itself. This is justified by its importance to radiation theory and by the large and peculiar variations in amount both with time and place. The chapter calls for little comment, being both full and clear.

Winds and Turbulence in the Stratosphere are next discussed in a short chapter. Up to about 30 km, balloons can be used and have demonstrated the monsoon effect predicted by F. J. W. Whipple, the winds being westerly in winter and easterly in summer around 20 to 30 km. At greater heights winds can be obtained from meteor trails and noctilucent clouds. It is of interest to note that winds at the level of these high clouds are nearly always from around ENE.

In the final chapter on Radiation the author comes to his own particular subject and it is discussed in much detail. This chapter is incidentally much the hardest reading of any. Unfortunately the general result is that at the present time our knowledge is not sufficient to allow any completely satisfactory theory to be formulated, though with the present rapid progress one may hope that this will soon be changed.

Summing up the whole book one may say that it gives an accurate account of present knowledge and the ways in which that knowledge has been obtained. One feels that there is some lack of balance (even for the physicist) and perhaps an expansion of the earlier chapters and corresponding slight contraction of the last chapter might have been better.

Finally no reviewer ought to close a review of a book in this series without a protest against the price charged by the publishers; 25s. for a book of 187 pages seems unduly high, even in these days, and must prevent many from buying, who would otherwise do so.

G.M.B.D.

Electrical breakdown of gases. By J. M. Meek and J. D. Craggs. Oxford (Clarendon Press), 1953. Pp. vii, 507; 320 Figs., 82 tables. 60s.

The main interest of this book for the meteorologist is the chapter on the lightning discharge. This is a useful summary of the information available concerning the discharge itself, though one which may be misleading on one or two aspects. For example, it is doubtful if experiments with long sparks on models in the laboratory are at all relevant to the consideration of the protective range of a lightning conductor or the existence or location of lightning nests. Reference might have been made too to recent work at Cambridge which indicates that the stepped structure of first leaders may not be as prevalent as had been believed. The importance of the photographic evidence on lightning obtained with the Boys camera is emphasized by the fact that even in such a treatise a large part of the discussion derives from it. But it is curious how often the early German photographs, which first demonstrated the composite nature of the lightning (Kayser 1884) and spark (Walter 1898) discharges, are overlooked.

For the rest the book forms a very useful compilation, as can be judged from the number of figures and tables, and the author references, which run to well over nine hundred, while each chapter is followed by an extensive bibliography. A long introductory account of the fundamental processes in electrical discharges is followed by chapters on breakdown at low pressures and corona discharge. There are six chapters on aspects of the spark discharge, including that on lightning, and finally three supporting chapters on high-frequency breakdown, electrode effects and glow-to-arc transition. The more ample treatment of the last-mentioned transition than is usual in such treatises is a welcome innovation.

The discussion of the classical problem of the electric spark in a uniform field, which appropriately enough occupies the very core of the book, is largely based on the senior author's streamer theory. However, reference is made to recent work of Llewellyn Jones and others, which has shown that a development of the original Townsend theory will satisfactorily account for the experimental results over the whole range so far investigated.

Workers in the wide field covered will be grateful to the authors for having assembled so large a body of data between one pair of covers. That the book is beautifully produced goes without saying.

C.E.R.B.

Elementare Theorien der Linienböen und Schlotströmungen. By H. Koschmieder. Vol. 1, Flug-meteorologische Forschungsarbeiten. Brunswick (Fr. Vieweg). 1953. Pp. 88; 42 Figs. 2 tables.

This pamphlet seems to be the first number of a new series of research papers on meteorology applied to aviation which are to be edited by H. Koschmieder.

This first number describes attempts to derive mathematical models simulating the fields of flow in line-squalls and in the updraughts of cumulus clouds.

The first part gives kinematic models of the flow of an incompressible fluid in two dimensions representing the air flow in a long line-squall. The models are built up from two or three vortex-lines whose axes are along the length of the line-squall and whose relative positions and strengths are so chosen as to satisfy the boundary condition at the ground and produce ascending currents in regions where the cloud structure suggests they exist. The field of flow can be calculated and the lines of flow, both absolute and relative to the steady motion sideways of the vortex lines, are shown in clear figures. Comparisons are made with the descriptive schemes of G. C. Simpson, Möller and others, some of which are shown to agree well with the author's models. There is no discussion of thermodynamics.

The second part gives similar work on convection currents using models in which the vertical velocity is a sine function of the height and the flow is in rectangular cells or is produced by vortex rings with vertical axes. The effects of variation of air density with height and compressibility are taken into account approximately. Particular attention is given to the pressure variation in a rising column. The calculations for a vertical current having a maximum velocity of 27 m/sec in the middle of the troposphere and zero velocity at the tropopause give a maximum reduction of pressure, over the static value, of about 3 mb. There is little thermodynamics.

The author, in an eloquent preface, admits the incompleteness of the models but claims they describe the most important features.

G.A.B.

The Australian environment. Edited by N. S. Noble, Commonwealth Scientific and Industrial Research Organisation, Melbourne. Second edition 1950. Pp. 183; 35 Figs., 17 tables, many photographs. 10s. 6d.

It is rare for a handbook prepared for a special occasion to survive the occasion and justify reprinting as a worthwhile addition to scientific literature for general use. Here is such a rarity. The book was first prepared by the Australian C.S.I.R.O. for the delegates at a specialist agricultural conference on 'Plant and animal nutrition in relation to soil and climatic factors,' held in Australia in August 1949, and though admittedly over-simplified it does give a stimulating survey of the effect of weather and climate in shaping the pattern of Australian agriculture. Apart from its general appeal, it should be excellent supplementary reading for those interested in Australian geography or Australian meteorology, and there is little doubt that the conference delegates found, as your reviewer did two years later, that it is a first-class source of background information for anyone wanting to study problems of agricultural meteorology in Australia.

H.L.P.

Klima-Atlas von Baden-Württemberg. Bad Kissingen (Deutscher Wetterdienst), 1953. Pp. 37; 9 Figs, 75 charts. 30 DM.

This is the third of the series of climatic atlases of the western zone of Germany. The same series of 75 charts has been given as for the earlier numbers dealing with Hesse and the diagrams are also almost the same. The very high quality of the earlier numbers has been maintained.

R.S.R.

Studies on fogs; in relation to fog-preventing forest. Edited by T. Hori. Sapporo, Japan (Tanne Trading Co.), 1953. Pp. vi, 399; Figs., tables, plates.

This book is a collection of twenty-five papers by various authors, members of the Institute of Low Temperature Science of Hokkaido University, who have taken part in an extensive investigation of the sea fog which very frequently invades the south-eastern coasts of the island of Hokkaido in spring and summer. The investigation, begun in 1950, was planned to study the effects on the fog of the forest belt which has been preserved along the coast to provide shelter to the inland districts.

Introductory papers by Dr. Z. Yosida and the late Dr. K. Imahori, directors of the investigation, suggest that the forest affects the fog directly, by capturing droplets from the fog blowing through or over it, and indirectly, by increasing the turbulence of the air; increased turbulence will lead to greater deposition of fog drops on the ground behind the forest and to the more rapid transfer of heat from the warm ground inland to the cold foggy air. Succeeding theoretical papers derive equations describing these effects; they are all based on the assumption that eddy diffusivity is constant with height above the ground or forest.

The remaining two-thirds of the book describe the apparatus and results of the field investigations. Turbulent-wind velocities were measured by hot-wire and vane anemometers, the water content of the fog by collecting the droplets on wire screens exposed to the wind, the size of the droplets by microscopic examination after capture on oiled slides or by a photographic version of the corona method, and the wind force on a tree by measurements on one specially mounted. An electron-microscope study of condensation nuclei was also made.

The book as a whole lacks any connected survey of the results of the various researches and their interrelations – perhaps because the work is still continuing. It is not even clear, for example, whether it has been established that the forest has any significant effect on fog at all, nor are the experimental results used as critical checks on the various theories put forward. The apparatus described is often ingenious in detail but does not appear to embody any new principles. Its accuracy is not very adequately discussed; several obvious sources of error are not mentioned, and checks on performance seem to have been few.

K.H.S.

Tornadoes of the United States. By Snowden D. Flora. Norman, U.S.A., University of Oklahoma Press. 1953. Pp. xiv, 194; 10 Figs., 6 tables, 16 plates. $3.50.

This book, by a retired senior meteorologist of the U.S. Weather Bureau, who for over 30 years was section director of the Climatological Service of the Weather Bureau, provides a welcome addition to our knowledge of tornadoes. Living in Tapeka, Kansas, where on the average, 18 tornadoes occur yearly, he is well fitted to speak from personal experience.

The author sets himself to summarize for the ordinary citizen the main facts about tornadoes – where and when they occur, what damage they do, how they are forecast and what precautions should be taken to avoid injury.

The book is essentially a compilation rather than a scientific treatise and the causes of tornadoes and the synoptic conditions preceding them are only briefly dealt with. For residents in the United States it does however succeed in bringing together a vast number of facts and figures which until now have been scattered in scientific periodicals and local newspapers. Nearly half the book is occupied by Chapter 8, descriptions of outstanding tornadoes that have affected large cities, Chapter 9, an assessment of tornado hazard by States, and Appendix I, a chronological list of outstanding tornadoes. Statistics of any small local phenomenon are exceptionally difficult to handle and when one reads a statement that ' a person living 68 years in Arkansas has one chance in 1,490 of being killed . . . by a tornado ' one cannot help wondering whether if the population were doubled one's chance of survival would be affected.

The author does not seem to have had access to a good meteorological library, and is on less firm ground when he deals with tornadoes in other parts of the world. The storm in Holland of which he says ' no details are available ' was the subject of an intensive investigation by the Meteorological Service of the Netherlands culminating in a paper by its Director (*K. Akad. Wetens. Amsterdam Proc.*, **28**, pp. 871-89, 1925) and a note on tornadoes in Australia was included in a *Bulletin of the Bureau of Meteorology* at Melbourne as long ago as 1925. The difference in local meaning that can be attached to such well known terms as ' cyclone ' is brought home very clearly; the statement, ' The average person is usually unaware of the passage of a cyclone unless he has access to a barometer or a current weather map ' would hardly ring true to a resident on the shores of the Bay of Bengal.

The book is well illustrated with photographs both of tornado clouds and the damage which is caused. The index, a problem to any author, seems to make an odd selection: there is an entry, ' Chickens stripped of feathers,' but nothing on rainfall or precipitation. These however are minor defects and the author is to be congratulated on the good use to which he has put his years of retirement.

E.E.A.

Glacier variations and climatic fluctuations. By H. W : son Ahlmann. Bowman Memorial Lecture, Series 3. New York (American Geographical Society), 1953. Pp. v, 51; maps, diagrams, photographs. $2.50.

It is a Cumberland proverb that good stuff wraps up in little bundles; truly that can be said of this reprint, beautifully produced, of the attractively comprehensive lecture given by Dr. Ahlmann to the International Geographical Congress at Washington in 1952, under the auspices of the Isaiah Bowman Memorial

Fund. Meteorologists in this country will be aware of the studies of the factors governing glacier advance and retreat initiated by Dr. Ahlmann in Norway in 1922, and developed over many years by himself and his younger colleagues not only round the North Atlantic but in the Antarctic as well; while the stimulus provided by his work has led others to carry out similar investigations in North and South America and elsewhere. It has of course happened that these studies have coincided with a period of widespread climatic amelioration sufficiently marked to attract a great deal of attention in northern lands. Relationships between glacier behaviour and temperature fluctuation can now be studied for the first time in some detail and the findings can be used as a basis for discussion of events in the past.

Previous summaries of his work have been given by Dr. Ahlmann in this country, notably in the first *Research Publication of the Royal Geographical Society* (1948), reviewed in this Journal in 1949 (**75**, p. 111). In this latest lecture however he has incorporated some findings from the Norwegian-Swedish-British Antarctic party's work at Maudheim and beyond. For example it now appears that the volume of the earth's ice is greater than was formerly thought. He emphasizes the need for caution before we embark too rapidly on the utilization of glaciers as climatological registers; glaciers for example do not all react with equal speed to climatic events. He incorporates the results of several recent papers dealing with fluctuations during the past 2-3 centuries; the correspondence in general behaviour between groups of glaciers situated in regions far apart is striking. To quote one instance : Lawrence, using tree-ring counts for his dating, confirms that the greatest advance of the glaciers from the southern part of the Juneau Ice Field in Alaska culminated sometime in the early or middle 18th century and that recession began by 1765 at the latest; the extent of the agreement with events in Iceland and Norway is noteworthy. Associated studies by Willett and Lysgaard of recent climatic fluctuations are familiar to this Society (*Centenary Proceedings*, 1950, and elsewhere).

Dr. Ahlmann goes on to review many minor results of the recent fluctuation with regard to animal and plant life, and to indicate its dimensions in relation to what we know of post-glacial and historic events. While in most districts round the North Atlantic all the evidence still goes to show that the greatest post-glacial advances culminated between 1750 and 1900, occasional older morainic ridges have been reported from certain districts which now suggest that in early sub-atlantic time a few glaciers may have advanced a little further. An appropriately cautious view is taken of the prospect, imminent according to some, of climatic recession to offset the amelioration since 1925. The photographs illustrate recent events in Scandinavia; that of the increased growth of vegetation near the tree-line is noteworthy. There is a very extensive list of references; printing and production are elegant; and the lecture provides a welcome and compact summary of progress in a large and ever-ramifying subject.

G.M.

The climates of the continents. By W. G. Kendrew. Oxford (Clarendon Press). 4th Edition, 1953. Pp. 607; 194 figures, tables. 50s.

In the preface to the first edition of this book, published over thirty years ago, the author stated ' there is no adequate description of the actual climates of the countries of the Earth, considered regionally, available in English.' Although this is not quite true today, the new (fourth) edition, owing to its method of presenting the facts of world climate in an easily readable and compact form, will in the reviewer's opinion continue to be a standard work of reference.

Each continent is introduced by a section entitled ' General Features;' this discusses the pressure and wind distribution, based on mean monthly sea-level isobars, then the air masses and oceanic conditions affecting the region (maps showing ocean currents would be helpful); the temperature and rainfall regimes are then described in detail and in some cases sections have been added on snow, cloud, sunshine, visibility, and humidity; it is thought that rather more attention should be given to humidity, concerning which there is a great and increasing demand for information, from industry.

The discussion is generally on more modern lines than the third edition, e.g., the rainfall regimes are described with reference to the position and movements of the Intertropical Front; synoptic charts are used to show some typical situations, e.g., a chart showing a family of depressions in the Atlantic, with their frontal systems, another showing the advance of a cold front into Brazil and a third illustrating ' berg winds ' in the Cape Peninsula. Many of the data used are more recent and reliable, and references have been made to modern climatological atlases. Recent work has been consulted, e.g., in the explanation of the dryness of the deserts of Sind and Thar, Sawyer's paper in the *Quarterly Journal* (1947) is quoted.

The text, when it involves statistical details, is often relieved by a well-chosen quotation describing some aspects of the climate experienced by a traveller; useful too are the references to the agriculture and vegetation of a region and their association with the climate; this is especially valuable as they can give useful indications of the climate where observations are scarce (e.g., Tibet).

The effects of latitude, altitude, ' continentality,' mountain ranges, seas, lakes and rivers, are continually illustrated throughout the book; local winds and tropical cyclones are dealt with in some detail including apt quotations describing their harmful effects; brief but useful summaries are given of major climatic regions. The seasonal changes of climate are explained as far as possible and a more dynamic picture has been given of world climate in general than in the last edition.

Several regions, especially East Africa, Indonesia and Canada, have received fuller treatment; very little has been added to the last section on Antarctica but this should be remedied in a future edition by the fact

that observations over the last ten years in the Southern Ocean and near the coasts of Antarctica are in course of publication.

Very few errors or misprints were found in the text. In reading a work such as this, where frequent references must be made to an atlas, the reviewer felt it would be a great boon if agreement or standardisation could be obtained regarding place names over the world and even some guide given to their local pronunciation.

J. Pepper

Climatology. By A. Austin Miller. London (Methuen). 8th Edition, 1953. Pp. x, 318; 82 figures, tables. 21s.

This is a new edition of a deservedly popular text-book handsomely reset with bold sub-headings which facilitate reference. A rather smaller type has made it possible to include some additional material on fewer pages but there is no loss of legibility.

The additions are not extensive. Apart from the bold but interesting attempt to map the principal causes of rainfall (Fig. 27) they are concerned mainly with a defence of the relatively simple system of classification employed. Professor Miller is aware that a genetic classification of climate on the basis of air-mass qualities may yet be possible. He also recognizes that the medical man, plant ecologist and engineer each approaches climatology with his own particular scale of reference. What is required by the geographer, however, is ' the recognition of major regions on a sub-continental scale,' – this can be achieved by a ' classification of convenience ' where simple definitions are both logical and desirable. Miller's new definitions of the limits of arctic and desert climates are good illustrations of this point. Their introduction has involved little change in the content of the regional chapters.

One might have hoped indeed for a rather more extensive revision in view of the evident progress of climatology since 1931. Thus, although a section on atmospheric stability was introduced into the text in 1944, the fundamental importance of inversions aloft in the mechanism of drought in West Africa (p. 109) the East Indies (p. 113), and Sind (p. 151) and in the meteorology of the trade-wind deserts in general (pp. 251-3) is nowhere clearly indicated. A gingerly treatment of the general circulation is perhaps desirable in a work of this standard but it is certainly not true that the ' trades ' are strongest at Honolulu in winter (p. 119) and the error of ' lower ' for higher latitudes on p. 60 has survived repeated editions. There are some unhappy expressions, particularly in relation to oceanography : ' The North Atlantic Drift banking up . . . a depth of warm water ' (p. 45) and the ' Equator-seeking return current ' (p. 253) – for the most active element in the circulation of the South Atlantic – are both phrases born to plague the weary examiner for years to come. Teachers have also found difficulty with the use of the word ' steep ' in reference to lapse rates (p. 56) especially when accompanied by Fig. 17 where a ' steep ' lapse rate involves a curve of low gradient. Those, but only those, who have heard Professor Miller lecture brilliantly on common geographical misdemeanours will know that he is now fully conscious of the error of such ways. It is a pity he did not re-read his text when it came of age.

The tabulated data have not been revised but there has been a considerable increase in the scope of the references.

In view of the price of its nearest competitor this book remains very good value for money.

P.R.C.

The measurement of plant climate. By Sidney A. Searle. Chichester (Chichester Press Ltd.), 1953. Pp. 36; 6 figs., 13 plates, 2 tables. 4s. 6d.

This second booklet by Mr. Searle on climate and horticulture deals in particular with earth temperature at one foot below a turf surface, which he considers an efficient and reasonably adequate measure of ' plant climate.' The diurnal variation of earth temperature, the influence of other factors (e.g., precipitation, sunshine, vegetative cover, etc.) are briefly discussed, and maps are given of the mean monthly and annual values of earth temperatures, at depths of one foot and four feet respectively, over most of the British Isles (corrections having been applied for altitude and for diurnal variation).

Then follows a description of a one-foot right-angled earth thermometer embodying certain novel and interesting features. The possible uses of such an instrument in daily horticulture practice are discussed and illustrated by reference to the exceptionally low earth temperatures from September 1952 to March 1953.

Earth temperature is a most useful parameter, but the author rather overstates its importance especially (p. 21) when the possibility of forecasting is envisaged, even if only temperature is to be considered. The maps of earth temperature deserve close study, although it is necessary to justify applying directly to earth temperature, the reduction for height (1°F per 280 ft), conventionally applied to air temperature. The *thermal diffusivity* of soil does not increase with moisture content except when the soil is dry and a comment on p. 7 is misleading.

One also wonders whether one foot may not be too far below the level of germinating seeds and surface root systems to respond to those large temperature variations at shallower depths, which are so important to plant and insect life and which in large measure account for the distinction between ' light ' and ' heavy ' soils.

In spite of some criticisms, however, the booklet may be recommended to a wide public.

R.W.G.

Obituary Notice

Mr. R. S. WHIPPLE, M.I.E.E., F.Inst.P.

A Life Fellow of 33 years' standing was lost to the Society by the death on 13 December 1953 at his home in Highgate Village, London, of Robert Stewart Whipple, the well-known authority on the history, development and manufacture of scientific instruments. Born on 1 August 1871, he received his education at King's College School, London, and in 1888 was appointed an assistant at Kew Observatory, of which his father, G. M. Whipple, had been superintendent since 1876. G. M. Whipple died in 1893 and was succeeded by Charles Chree, under whom the son continued to serve at the observatory for three years. In 1896 he left to join the staff of L. P. Casella, and in 1898 he became personal assistant to Horace Darwin, co-founder with A. G. Dew-Smith in 1881 of the Cambridge Instrument Company. A few months later Robert Whipple was appointed manager and secretary of this company. In 1909 he was made joint managing director, a post which he occupied with distinction for more than a quarter of a century. He remained a director after retiring from active management in 1935 and over the period 1939-49 he was chairman of the Board.

Whipple was a prominent and valued member of many bodies concerned with physics, industrially and otherwise. He served on two occasions as president of the Scientific Instrument Manufacturers' Association, as president of the Optical Society in 1920-22, as a vice-president of the Physical Society from 1914 to 1916 and as its treasurer from 1925 until 1935. For a total of 21 years he was on the Board of the Institute of Physics, of which he had become a founder member in 1920. In 1936-37 he was honoured by appointment as Faraday Lecturer to the Institution of Electrical Engineers, and in 1939 by election to presidency of Section A of the British Association for the Dundee meeting which had to be abandoned because of the outbreak of war during the autumn of that year.

Whipple was one of those fortunate men whose profession is also their chosen hobby. From youth onwards his main interest lay in studying the development of scientific apparatus in all its forms. As the years passed he gathered together a wonderfully comprehensive collection of historic instruments and relevant literature. This he generously presented, in 1944, to the University of Cambridge, where it is now housed in a small building known as the Whipple Museum of the History of Science. He added handsomely to the gift by transferring to trustees funds eventually amounting to some £8,000 for use in supplementing the collection by the purchase of further instruments and books. To Whipple belongs the credit for introducing into this country the Féry radiation recorder. He himself designed an instrument, widely employed in modern industry, for reading temperatures direct on the gas scale.

One of his last acts was to provide for a fine historical tablet giving the list of Superintendents of Kew Observatory to be placed in the appropriate room there. A simple ceremony was planned for its unveiling by Sir George Simpson, but had to be deferred owing to Whipple's illness. The tablet is now in position and it is hoped to arrange its formal unveiling in the coming summer.

Robert Whipple's younger brother, the late Francis John Welsh Whipple, succeeded Dr. Charles Chree as superintendent of Kew Observatory in 1925 and was president of this Society in 1936-38.

R. S. Whipple was a gentle soul in voice, manner and spirit. The large congregation at the funeral service in St. Michael's Church, Highgate Village, on 16 December 1953 was an indication of the esteem and affection in which he was held, both in the world of science and by the staff and workpeople of the Cambridge Instrument Company, a representative body of whom came from Cambridge on that bleak December morning to share in the service. The Society was represented by Sir George Simpson, K.C.B., C.B.E., D.Sc., F.R.S., and Mr. E. Gold, C.B., D.S.O., F.R.S.

E.G.
E.L.H.

THE NAPIER SHAW MEMORIAL PRIZE

Announcement of first competition

The Royal Meteorological Society announces the first competition for the Napier Shaw Prize. The competition will be open to anyone without restriction of nationality. The prize of £100 is offered on this occasion for an original essay on **'The energetics of the general circulation.'**

> The essay will be expected to treat of the energy transformations of the circulation as a general theme or any particular aspects of that theme. It may be an essay in which the author describes and discusses the salient and essential features of the general circulation and the energy transformations involved; it may be an essay which attempts an adequate synopsis and discussion of the observations, or a study of the thermodynamics and hydrodynamics of the circulation; or the essay may treat of any combination of these within the general theme.

The essay, which must be in English, may include material which the candidate has already published. Essays may be submitted jointly in the names of more than one author.

Candidates are required to submit three copies of their essay, which must be printed or typewritten. In the latter case, only one side of the paper should be used, double spacing should be employed and two of the copies may be good carbon copies. Unless the essay is printed, any mathematics should be written in carefully by hand. Although the scale of the essay will be considerably greater than that of an ordinary paper, candidates are recommended to consult, for general advice, the pamphlet 'Preparation of papers for the Quarterly Journal.' This may be obtained, free of charge, on application to the Assistant Secretary.

It is required that any essay for which a prize is awarded shall be offered to the Society for publication.

All entries for the competition should be addressed to the Assistant Secretary, Royal Meteorological Society, 49 Cromwell Road, London, S.W.7 and marked clearly 'Napier Shaw Prize.'

Essays for the competition must be received at the Society's Offices *not later* than 4 March 1956.

Announcement

PROCEEDINGS OF TORONTO METEOROLOGICAL CONFERENCE 1953

By arrangement with the American Meteorological Society the Proceedings of the Toronto Conference, which was held in September 1953, will be published by the Society in the format of the *Quarterly Journal*.

It will be seen from the summary of the contents given below that the papers cover a wide range of meteorological research and that many are by authors of international repute.

CONTENTS

DISCUSSION ON OZONE AND THE HIGH ATMOSPHERE

Chairman : Sir Charles Normand

Opening address by Sir Charles Normand ; ' Atmospheric winds and temperatures to 60-km altitude as determined by acoustical propagation studies,' A. P. Crary, W. B. Kennedy and V. C. Bushnell; ' Atmospheric temperatures at 30-km,' A. Arnold and C. J. Brasefield; ' Rocket observations of atmospheric ozone,' F. S. Johnson; ' An investigation of the 9·6 μ ozone band in the telluric spectrum,' R. M. Goody and C. D. Walshaw.

SYMPOSIUM ON RADIATION AND OTHER ENERGY TRANSFORMATIONS

Chairman : Dr. G. D. Robinson

' Spectral models and properties of transmission functions,' W. L. Godson; ' A quasi-statistical approach to the calculation of atmospheric transmission,' L. D. Kaplan; ' Absorption by the 9·6 μ band of ozone,' C. D. Walshaw and R. M. Goody; ' Some problems in atmospheric radiation,' G. N. Plass; ' Distribution of infra-red cooling in the atmosphere for winter and summer seasons,' J. London.

SYMPOSIUM ON ARCTIC METEOROLOGY

Chairman : A. Thomson

' Some problems in arctic meteorology,' H. U. Sverdrup; ' Forecasting problems in the Arctic with special reference to northern Greenland,' M. J. Toyli; ' Radiation and wind and temperature profiles over an antarctic snow field,' G. H. Liljequist; ' The problems of establishing and maintaining weather stations in the Arctic,' R. W. Rae; ' Arctic track charts,' F. A. Berry, G. U. Owens, H. P. Wilson.

SYMPOSIUM ON GENERAL CIRCULATION

Chairman : Prof. P. A. Sheppard

' Momentum flux and meridional motion in the general circulation,' P. A. Sheppard; ' Some aspects of the mean upper-air flow over the earth,' J. K. Bannon.

SYMPOSIUM ON MODERN CYCLONE THEORY

Chairman : Prof. S. Petterssen

' The problem of cyclogenesis from Helmholtz's time to the present,' J. Bjerknes; ' Cyclones and anticyclones – a comparative study,' R. C. Sutcliffe; ' Some aspects of mobile depressions in a baroclinic current studied by means of the two-parameter representation of the atmosphere,' J. S. Sawyer.

Symposium on High-level forecasting

Chairman: Major General W. O. Senter

'Forecasting winds up to the 100-mb level by the contour chart technique,' J. S. Sawyer and R. C. Sutcliffe; 'Forecasting high clouds from high-level constant pressure charts,' J. E. French and K. R. Johannessen; 'Jet streams in middle and high latitudes,' D. P. McIntyre and R. Lee; 'Observations on billow clouds and deforming exhaust trails from aircraft,' R. C. Bundgaard; 'Jet-stream detail with respect to other meteorological factors,' S. Teweles, Jr.; 'Clear-air turbulence at high levels,' LeRoy H. Clem.

Panel discussion on Climatic change

Chairman: Prof. Gordon Manley

Introductory survey by Prof. Gordon Manley; 'Temperature trends in Canada,' Richmond W. Longley; 'Evidence of climatic variation from the deep-sea floor of the Atlantic ocean,' C. D. Ovey.

Symposium on Microclimatology and micrometeorology

Chairman: Dr. H. E. Landsberg

Introductory address by Dr. H. E. Landsberg; 'Topoclimatology,' C. W. Thornthwaite; 'Micrometeorology in the potato crop,' J. M. Hirst, I. F. Long and H. L. Penman; 'Some measurements of the shearing stress and its variation with height in the lowest 100 ft of the atmosphere,' E. L. Deacon; 'Atmospheric pollution in relation to microclimatology and micrometeorology,' E. W. Hewson.

Symposium on Cloud physics and induced precipitation

Chairman: Prof. H. G. Houghton

'Factors governing the temperature of ice crystal formation in clouds,' C. L. Hosler; 'Observations and measurements on natural and artificial rain from convective clouds,' H. R. Byers, L. J. Battan and R. R. Braham; 'An analysis of supercooled cloud characteristics measured during Canadian icing experiments 1950-53,' K. G. Pettit; 'Growth of hydrometeors as calculated from aircraft and radar observations,' R. M. Cunningham and D. Atlas.

The volume will contain numerous illustrations and plates and will be about 300 pages in length. Owing to the very heavy costs of production it is regretted that the Society cannot supply the *Proceedings* free to Members or existing subscribers to the *Quarterly Journal*, and the published price will be 30s. or $4.50, plus postage. The price to Members of both the Royal and American Meteorological Societies will be half the published price.

Orders which are placed in bulk with the Society by 30 June for not less than 12 copies will be charged at the special rate of £1 or $3 per copy post free.

An order form is enclosed with this issue and its early return would greatly help the Society in planning distribution, which it is hoped to commence by 30 June.

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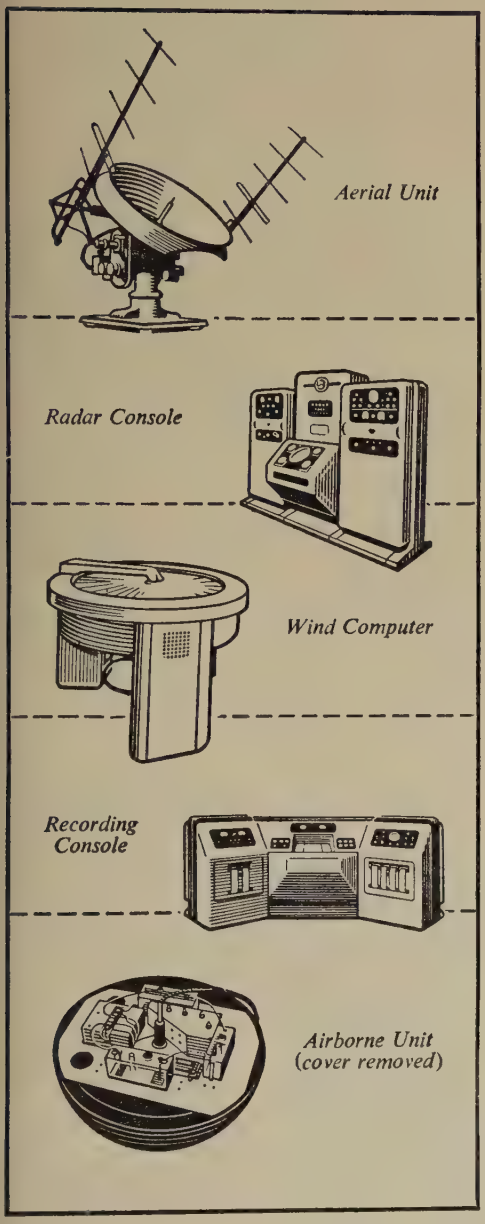

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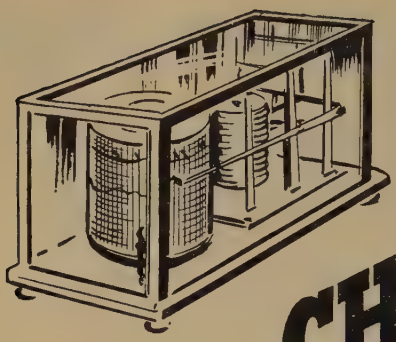

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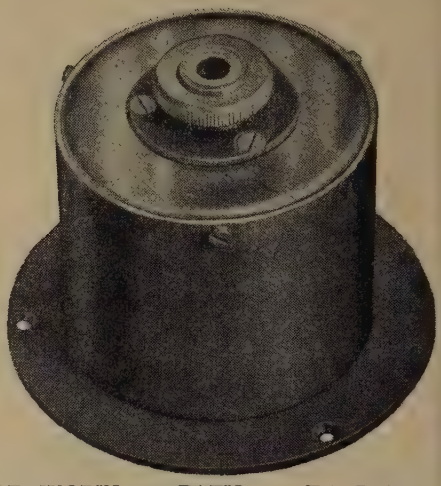

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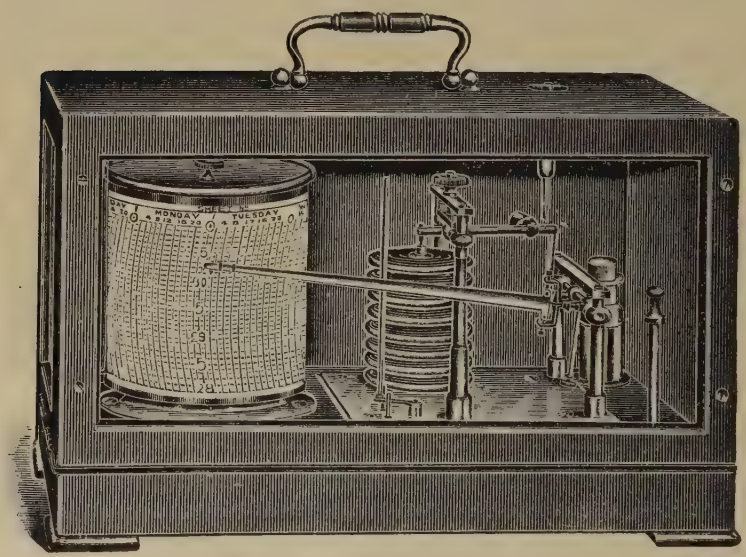

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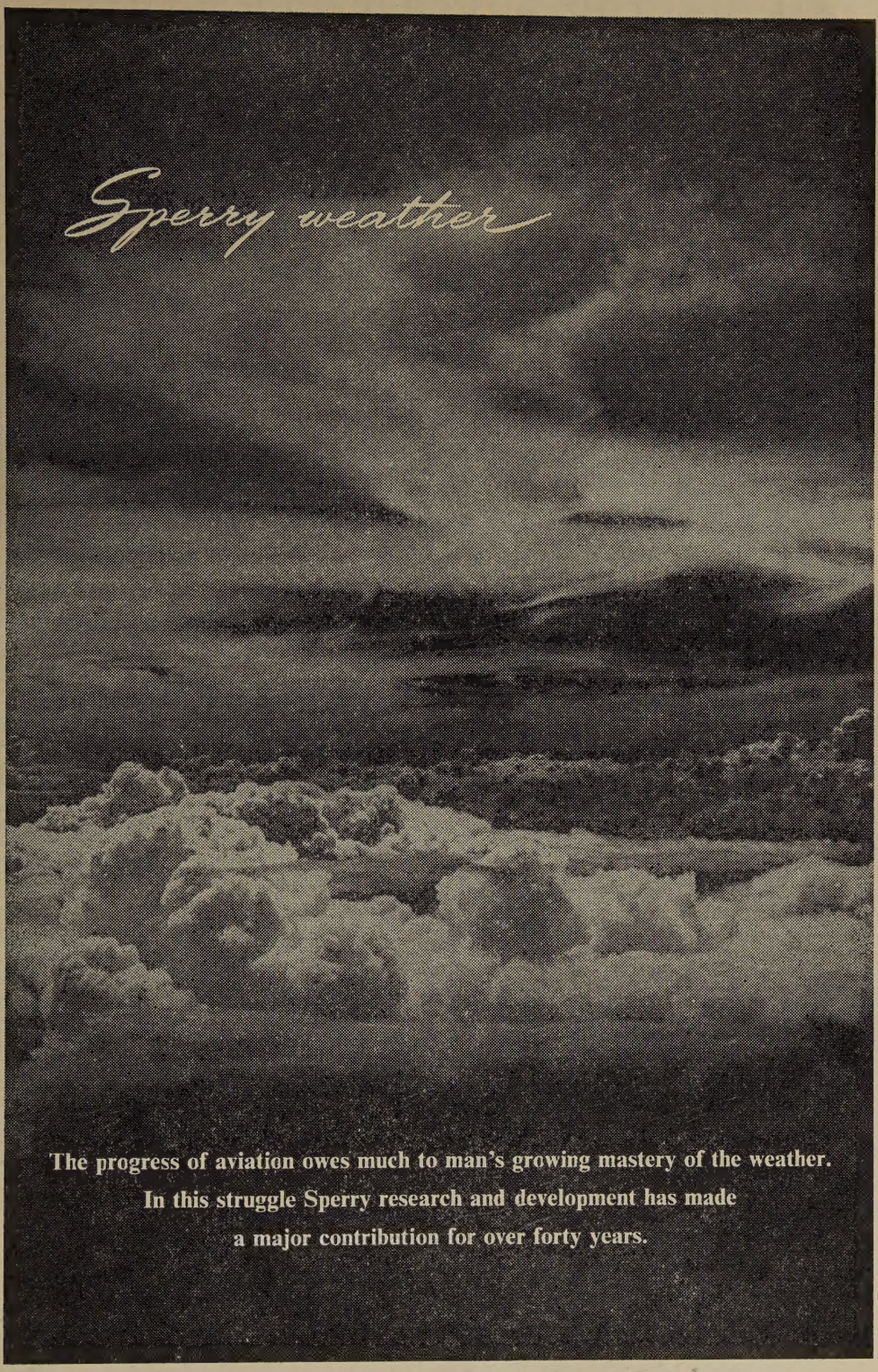
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